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**PHYSICAL SCIENCES**

Volume 4



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# PHYSICAL SCIENCES

Volume 4

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S.L. = 3888.  
REFERENCE



ELSEVIER PUBLISHING COMPANY LTD

ELSEVIER PUBLISHING COMPANY LIMITED  
BARKING, ESSEX, ENGLAND

ELSEVIER PUBLISHING COMPANY  
335 JAN VAN GALENSTRAAT, P.O. BOX 211, AMSTERDAM,  
THE NETHERLANDS

AMERICAN ELSEVIER PUBLISHING COMPANY INC.  
52 VANDERBILT AVENUE, NEW YORK N.Y. 10017

ELSEVIER PUBLISHING COMPANY LIMITED

444 20048-7

LIBRARY OF CONGRESS CATALOG CARD NUMBER 70-95326

# CONTENTS

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## 1890

The Scientific Work of Joule—James Dewar . . . .	1
Magnetic Phenomena —Shelford Bidwell . . . .	7
The Glow of Phosphorus—T. E. Thorpe . . . .	13
Electromagnetic Radiation - G. F. Fitz Gerald . . . .	18
Foam —Lord Rayleigh . . . . .	26
The Photographic Image —Raphael Meldosa . . . .	39

## 1891

The Rejuvenescence of Crystals John. W. Judd . . . .	50
Some Applications of Photography—Lord Rayleigh . . . .	58
Electro-magnetic Repulsion —J. A. Fleming . . . .	72
Electric and Magnetic Screening - Sir William Thomson . . . .	93
Liquids and Gases—W. Ramsey . . . . .	104
Crystallisation -G. D. Liveing . . . . .	114
The Molecular Process in Magnetic Induction --J A. Ewing . . . . .	127
The Rate of Explosions in Gases—Harold B. Dixon . . . .	142
The Faraday Centenary: H.R.H. The Prince of Wales and James Dewar . . . . .	150

## 1892

Alternate Currents of High Potential and High Frequency — Nikola Tesla . . . . .	167
Optical Projection—Sir David Salomons . . . . .	183
The Motion of the Ether near the Earth—Oliver Lodge . . . .	189
Metallic Carbonyls—Ludwig Mond . . . . .	205
Magnetic Properties of Liquid Oxygen—James Dewar . . . .	218

## 1893

Liquid Atmospheric Air—James Dewar . . . . .	223
Turacin, a remarkable Animal Pigment containing Copper—A. H. Church . . . . .	235
Interference Bands and their Applications—Lord Ray- leigh . . . . .	241
Study of Fluid Motion by means of Coloured Bands— Osborne Reynolds . . . . .	248

## 1894

Scientific Uses of Liquid Air—James Dewar . . . . .	258
The Scientific Work of Tyndall—Lord Rayleigh . . . . .	273
Electric Discharge through Gases—J. J. Thomson . . . . .	282
The Splash of a Drop and Allied Phenomena—A. M. Worthington . . . . .	291
The Work of Hertz—Oliver Lodge . . . . .	321
The Newtonian Constant of Gravitation—C. Vernon Boys . . . . .	350

## 1895

Phosphorescence and Photographic Action at the Temper- ature of Boiling Liquid Air—James Dewar . . . . .	375
The Physical Work of von Helmholtz—A. W. Rucker . . . . .	381
Argon—Lord Rayleigh . . . . .	397
The Effects of Electric Currents in Iron on its Magnetisa- tion—John Hopkinson . . . . .	412
The Absolute Measurement of Electrical Resistance— J. Viriamu Jones . . . . .	426

## 1896

More about Argon—Lord Rayleigh . . . . .	447
New Researches on Liquid Air—James Dewar . . . . .	457

Colour Photography—G. Lippmann . . . . .	479
Electric Shadows and Luminescence—Silvanus P. Thompson . . . . .	485
Hysteresis—J. A. Ewing . . . . .	511
Electric and Magnetic Research at Low Temperatures—J. A. Fleming . . . . .	517





Friday, January 24, 1890.

SIR FREDERICK ABEL, C.B. D.C.L. F.R.S. Vice President,  
in the Chair.

PROFESSOR DEWAR, M.A. F.R.S. *M.R.I.*

*The Scientific Work of Joule.*

(Abstract.)

PROF. DEWAR commenced by remarking that the Royal Institution had been so closely identified with the great workers in physical science that it was impossible to allow the work of Joule, whose researches had produced as marked a revolution in Physical Science as Darwin's in Biology, to pass without recognition in the present series of Friday Evening Discourses. Sir William Thomson, as Joule's friend and fellow-worker to the last, had been invited to undertake the duty, and had agreed to do so; but at the last moment had been compelled to decline by reason of important official duties in Scotland, and the task had consequently devolved upon him.

Having given a brief account of Joule's parentage, early life, and education, Prof. Dewar reviewed, as fully as time would permit, his scientific work, which extended over about forty years, and was represented by 115 original memoirs. The first period (1838 to 1843) was distinguished as that in which Joule educated himself in experimental methods, chiefly in connection with electricity and electro-magnetic engines. This work led him in 1840 to his first great discovery, the true law governing the relation between electric energy and thermal evolution, which enabled him later on to account for the whole distribution of the current, not only in the battery in which it is produced, but in conductors exterior to it. Joule was thus led to take up the study of electrolysis. Faraday had already made the discovery that electrolytic bodies could be split up into equivalent proportions by the passage of the same electric current; Joule saw that there would be great difficulty in finding out the distribution of the current energy, and accounting for the whole of it. After a laborious research he succeeded in showing that during electrolytic action there was an absorption of heat equivalent to the heat evolved during the original combination of the constituents of the compound body. The prosecution of his electrical researches rapidly brought Joule on the road to his great discovery of the Mechanical Equivalent of Heat, it being clear from a foot-note to a paper dated 18th February, 1843, that he already had well in hand the study of the strict relations between chemical, electrical, and mechanical effects.

In working out these laws, it was to be remarked that Joule—in common with most inventors and seekers after new scientific truths—chose perhaps the most difficult means that could have been selected;

and in looking back at his work in the light of present knowledge, it seemed simply astounding that he should have succeeded so completely as he did. The original coil used by Joule for the mechanical determination of heat (kindly lent for the occasion by Prof. Rücker) was shown, and the course of the experiment explained. The vast difficulties which Joule had to overcome in order to prove that there was a definite, permanent, and persistent relation between the amount of mechanical energy expended and the heat produced were commented on; the thermal effects being produced not directly but through the medium of an electric current varying in intensity, and calculations having to be made not only for these fluctuations, but for the effects of radiation, the movement of the air, and other indirect complications. The very small increment of heat to be measured obliged Joule to use thermometers of great delicacy, and these he had to devise and construct himself. One of the thermometers so used was exhibited.

Working in this way, Joule was able by the end of July, 1843, to state definitely that the amount of heat capable of increasing the temperature of a pound of water by  $1^{\circ}$  F. was equal to, and might be converted into, a mechanical force capable of raising 838 lbs. to the height of one foot. Soon afterwards he attained almost identical results by a more direct method—the friction of water passing through small tubes—which gave him 770 foot pounds per unit of heat.

It was impossible, said the lecturer, to thoroughly appreciate Joule's work without glancing at the early history of the subject; and when one did so it was amazing to find how near men of the stamp of Rumford, Davy, and Young had been to Joule's great discovery, and yet missed it. Count Rumford was the first to clearly define the relation between the constant production of heat and loss of movement by frictional motion. He proved that the amount of heat produced by friction was continuous, and apparently unlimited; but he did not think of measuring the relation between the mechanical energy expended and the amount of heat produced. Alluding to the results obtained from this apparatus, the lecturer said that Count Rumford might have shown that in his experiments the heat produced was proportional to the time of working, and so obtained a result capable of being expressed in horse-power. The value so deducted from Rumford's experiments is not far removed from Joule's first number.

The experiments commenced by Count Rumford were carried on by Davy, at that time working with Beddoes at Bristol; and led to one of the most remarkable essays on heat of that period, which disposed for ever of the theory of the separate existence of caloric. Taking two pieces of ice on a cold day, Davy mounted them so that they could be rotated against each other with frictional pressure, the effect being that the pieces of ice were melted, and the water so produced had a much higher specific heat than the original ice. To guard against the possibility of heat being conveyed to the frictional

apparatus by the surrounding air, Davy made an experiment in vacuo, isolating the apparatus by means of ice; and found that under such conditions sufficient heat could be produced to melt wax placed in the receiver. The lecturer here showed an experiment illustrating the production of water by the friction of two pieces of ice in vacuo, under conditions of temperature much more severe than those of Davy's experiment.

Following Davy, Young devoted a great deal of attention to the subject, and by 1812 he and Davy had quite changed their opinions, and had adopted the view that heat and motion were convertible effects.

Having by July 1843 assured himself of the principle of his discovery, Joule now devoted himself to the elaboration of methods of working, modifying and repeating experiments in various ways, but always approaching nearer and nearer to exactness, as shown by the following Table of results:—

Joule's values of the MECHANICAL EQUIVALENT OF HEAT.

		Kilogramme metres.
Magneto-electric currents .. .. .	1843	460
Friction of water in tubes .. .. .	"	424·6
Diminution of heat produced in a battery current when the current produces work .. .. .	"	499
Compression of air .. .. .	1845	443·8
Expansion of air .. .. .	"	437·8
Friction of water .. .. .	"	488·3
" " " .. .. .	1847	428·9
" " " .. .. .	1850	423·9
" " mercury .. .. .	"	421·7
" " iron .. .. .	"	425·2
Heat developed in Daniel's cell .. .. .	"	419·5
" " in wire of known absolute resistance .. .. .	1867	429·5
Friction of water in calorimeter .. .. .	1873	423·9

Prof. Dewar here exhibited and explained the action of the original calorimeter used by Joule. It was seen to consist of a set of vanes which were made to revolve in water by the falling of known weights through a definite and known height, the heat produced being due (after making the necessary deduction for the friction due to the momentum of the weights) entirely to the friction of the fluid. It was found that whatever fluid was employed, the same definite results were obtained:—a production of heat in the liquid bearing a constant relation to the unit of mechanical energy expended. The extreme delicacy of Joule's apparatus, and the marvellous accuracy of his observations were shown by the fact that working with weights of 29 lbs. each, and repeating each observation 20 times, the total increase of temperature did not exceed half a degree Fahrenheit. In contrast to this the lecturer showed, by means of apparatus kindly lent by Prof. Ayrton, the method now employed for repeating Joule's work and arriving at substantially the same results by much simpler means

While continuing to work intermittently at his great discovery, Joule employed himself in the following years in elaborate investigations bearing upon the point of maximum density of water, specific gravity, and atomic volumes. An illustration of his method of determining maximum density was given by means of two large cylinders filled with water and connected by a narrow channel in which was placed a floating indicator. It was shown that the slightest variation in density of the water of either cylinder—variations far beyond the scope of the most delicate thermometer—set up currents which were immediately detected by the movement of the indicator, and that by this means it was quite possible to ascertain the exact temperature at which water attained its maximum density.

Joule's determinations of atomic volumes were marvellous at the time they were made, and were still interesting. Illustrations of his work in this direction were given by means of a solution of sugar, which was seen to occupy practically the same space as was occupied by an amount of water exactly equivalent to that combined in the carbohydrate. The carbon hypothetically combined with the water to form the sugar appearing to make no sensible difference to the volume; and in contrast to this was seen the enormous difference in volume brought about by dissolving two equal portions of soda carbonate, one portion being ordinary hydrated crystals and the other portion being anhydrous, in equal volumes of water.

Joule's last great research was carried out conjointly with Sir William Thomson, and occupied nearly ten years of laborious enquiry. Its chief object was to prove that in compressing a gas the amount of heat produced is equivalent to the work done, and independent of the mere fact of the approach of the particles. But Joule was desirous of amplifying the enquiry, and in fact the work might be divided into three sections: (1) the study of gases passing through narrow apertures; (2) the velocity attained by bodies passing through the air; and (3) the temperature ultimately attained by such moving bodies. With respect to 2 and 3, it was shown that a body rotating in the air at the rate of about 150 to 180 feet per second increased in temperature by nearly  $1^{\circ}$  F., and that this increase of temperature was definite for a given velocity, and independent of the size of the moving mass and the density of the gaseous medium. With regard to (1) the relation of gaseous pressure and volume to temperature, the researches of Regnault had already shown that the simple law of Marriotte and Boyle could not stand by itself; and Joule sought to modify it by the study of gases passing through very small tubes or porous bodies. The investigations were carried out at Manchester on a large scale, and were assisted by a Government grant. Steam engines were employed to maintain a current of gas at a constant temperature and pressure through long coils of pipe placed in water tanks. They proved that any difference of temperature in the gas brought about in its passage through the porous body must be due to work done by it, and that this difference of temperature varied for different gases, according to their constitution.

Working under the same conditions, hydrogen was shown to be reduced a small amount in temperature, air somewhat more (about  $0.3^{\circ}$ ), and carbonic acid a much greater amount. A repetition of Joule and Thomson's experiment was shown by means of a 100 feet coil of lead pipe, compressed hydrogen, air, and carbonic acid gas being employed, and the original results verified in each case. The effect of this research was to enable Joule and Thomson to formulate a great improvement on the gaseous laws; for instead of the product of the volume and pressure being strictly proportional to the absolute temperature, as it had been hitherto believed to be, they found that a new term was involved, which is equivalent to a constant divided by the absolute temperature and multiplied by the volume.

In conclusion, Prof. Dewar read the following letter, which he had received from Sir Lyon Playfair in response to his request for some reminiscences of Joule:—

DEAR DEWAR,

20th Jan., 1890.

You ask for some of my memories of Joule from 1842 to 1845, when I was Professor of Chemistry at the Royal Institution in Manchester. The great Dalton died in the autumn of 1844, and had long been President of the Manchester Philosophical Society. He naturally gave impulse to the study of science in that town, where there was an active band of young workers in research.

Joule was, even then, foremost among these; and the names of Binney, Williamson, Schunck, Angus Smith, Young, and others show that the spirit of scientific inquiry was active. We were also stimulated by the fact that Baron Liebig and Bunsen came to pay me visits during that time; they were men to excite research.

Joule was a man of singular simplicity and earnestness. We used to meet at each other's houses at supper, to help the progress of our work by discussion. Joule was an earnest worker, and was then engaged on his experiments on the mechanical equivalent of heat. He took me to his small laboratory to show me his experiments, and I of course quickly recognised that my young friend the brewer was a great philosopher. We jointly worked upon questions of far less importance than his great central discovery, but he was equally interested. I was very anxious that he should devote his life to science, and persuaded him to become candidate for the Professorship of Natural Philosophy at St. Andrews. He was on the point of securing this, but his slight personal deformity was an objection in the eyes of one of the electors; and St. Andrews lost the glory of having one of the greatest discoverers of our age.

When Joule first sent an account of his experiments to the Royal Society, the paper was referred, among others, to Sir Charles Wheatstone, who was my intimate personal friend. Wheatstone was an eminently fair man and a good judge, but the discovery did not then recommend itself to his mind. For a whole Sunday afternoon we walked on Barnes Common, discussing the experiments and their

consequences, if true, to science. But all my arguments were insufficient to convince my friend; and I fear that then the Royal Society did not appreciate and publish the researches. I write from memory only, for I know that, later, no society or institution honoured Joule more than the Royal Society and its members.

Not for one moment, however, did Joule hesitate in the accuracy of his experiments or his conclusions. He once suggested to me that we might take a trip together to the Falls of Niagara, not to look at its beauties, but to ascertain the difference of temperature of the water at the top and bottom of the fall. Of course the change of motion into heat was a necessary consequence of his views.

No more pleasant memory of my life remains than the fact that, side by side, at my lectures in the Royal Institution, used to sit the illustrious Dalton, with his beautiful face, so like that of Newton, and the keenly intelligent Joule. I can give no other explanation than the fact of organic chemistry being then a new science that two philosophers of such eminence should come to the lectures of a mere tyro in science. I used to look upon them as two types of the highest progress in science. Newton had introduced law, order, and number into the movements of masses of matter in the universe; Dalton introduced the same into the minute masses which we call atoms; and Joule, with a keen insight into the operations and correlation of forces, connected them together and showed their mutual equivalence.

I do not know whether these memories are of any use to you, but, such as they are, they are at your disposal for your lecture on the friend of my youth.

Yours sincerely,

LYON PLAYFAIR.

Friday, February 21, 1890.

JOHN RAE, M.D. LL.D. F.R.S. Vice-President, in the Chair.

SHELFORD BIDWELL, Esq. M.A. LL.B. F.R.S. *M.R.I.*

*Magnetic Phenomena.*

THE space around a magnet in which magnetic action is observed is called a field of magnetic force, or, more shortly, a magnetic field. Following Faraday's conception, we may specify a magnetic field by supposing it to be filled with a number of so-called "lines of force," the direction of the force (that along which a north pole is urged) being indicated by the direction of the lines, and its intensity by their concentration. In a uniform field of unit intensity, the lines of force are straight and parallel, and each line is exactly one centimetre distant from its nearest neighbour; so that, if a flat surface were held transversely to the direction of the lines, one line would pass through each square centimetre of the surface. In a weaker field the lines would be farther apart; in a stronger one they would be packed more closely together. The direction of the earth's magnetic force at any point in or near London is, roughly speaking, from south to north, at an inclination of  $67^{\circ}$  to the horizon; its intensity is approximately such that one line of force traverses every two square centimetres of a transverse plane surface, *i. e.* half a line, for each unit of area. The intensity of a unit field of magnetic force is therefore equal to about twice the total intensity of the magnetic field due to the earth.

It is a remarkable fact that iron, and in a less degree the two rarer metals nickel and cobalt, when placed in a magnetic field, possess the property of multiplying the number of lines that would naturally fill the space occupied by them. Thus, a long and thin iron rod placed lengthwise in the earth's magnetic field will not merely be traversed by half a line for each square centimetre of its section, as a glass or copper rod would be; the half line will (at least in the middle portion of the rod) be multiplied something like 600 times, raising the actual number of lines through the iron to about 300 per centimetre of section.

By means of electric currents it is easy to produce magnetic fields having a far higher intensity than that of the earth. Suppose, for example, we take a long brass tube, and wind around it a quantity of insulated copper wire, forming 16 convolutions in each centimetre of length; a current of 10 ampères circulating through such a coil would generate in the interior a magnetic field having an intensity of about 200 units. An iron rod placed inside this tube would be

traversed by perhaps as many as 18,000 lines per centimetre. Although this is a very large number, it will be noticed that it is smaller, in proportion, than was obtained when the magnetic field of the earth alone was employed. In that case a field of half a line to the centimetre was found to induce 300 lines in the iron, the multiplying power being 600. But with an external field of 200 the multiplying power is only about 90, a very considerable falling off. It is usual to denote the number of lines per square centimetre in the magnetic field by the letter  $H$ , and those induced in the iron by  $B$ , while the multiplier is indicated by the Greek letter  $\mu$ . We may therefore write:—

$$B = \mu H.$$

$B$  is commonly spoken of as the “magnetic induction,” and  $\mu$  as the “permeability.”

It used to be assumed that, except in strong fields, the permeability  $\mu$  was practically a constant for the same specimen of metal. We have already seen that this is by no means the case, and how very far it is from being so is clearly shown by the following table, in the first and third columns of which are given corresponding values of  $H$  and  $\mu$  for an average specimen of wrought iron.

TABLE I.—IRON.

$H$ Field.	$B (= \mu \times H)$ Induction.	$\mu$ Permeability.
0.2	80	400
0.5	300	600
1	1,400	1,400
2	4,800	2,400
4	8,800	2,200
7	11,200	1,600
11	13,200	1,200
16	14,400	900
65 (Rowland)	16,500	255
200 (Bidwell)	18,000	90
585	20,000	34
24,500 (Ewing)	45,300	1.9

It will be remarked that, as the strength of the field increases from the smallest values, the permeability at first rises with enormous rapidity, attaining in a field of 2 or 3 units a maximum value of more than 2000; then it falls again, rapidly at first, and afterwards more slowly, until with a field of 65 lines to the centimetre the permeability is no more than 255. So far, the figures in the table (which are given in round numbers) are based upon experiments made by Prof. Rowland sixteen years ago. Plotting corresponding values of  $\mu$  and  $B$ , Rowland constructed a curve, the form of which led him to the remarkable conclusion that the value of the magnetic induction  $B$  could not



possibly exceed a certain definite limit, and that, in fact, no magnetic force, however great, could induce in iron more than about 18,000 lines per centimetre. This conclusion, which seemed to be in agreement with Weber's theory of magnetism, was generally accepted as correct. Unfortunately, however, Rowland's experiment did not go quite far enough. If he had been able to carry his magnetising force a little beyond 65 units, he would have seen that there was no such limit as he supposed. More recently, an induction of 18,000 has been actually obtained with a field of only 200, the permeability being 90. With the stronger field of 585, the induction was found to be 20,000; and quite lately, Professor Ewing, employing a field of 24,500, has obtained an induction of 45,300, the permeability being 1.9. Ewing concludes that there is no limit whatever to the degree to which magnetic induction may be raised; and there can be no doubt that he is right.

But while Ewing's experiments tend to show that the number of magnetic lines which can conceivably be made to run through a piece of iron is indefinitely great, they at the same time clearly indicate that the number of *additional* lines in excess of those contained in the field before the iron was placed there, has a very definite limit. This limit, for the piece of wrought iron which he used, appears to have been about 21,000, and it was practically reached with an external field of about 2000. For this sample of iron we may, therefore, say that in fields of 2000 and upwards,

$$B = H + 21,000.$$

Closely connected with the questions which have just been discussed, are the further questions:—What are the conditions affecting the lifting-power of an electro-magnet? and, What is the greatest lifting-power attainable?

One point of fundamental importance was settled experimentally by Joule many years ago. He found that the power of a uniform electro-magnet varies directly as the sectional area of the iron core, so that, for example, a magnet with a section of two square inches would, other things being equal, carry twice the weight that could be supported by one with a section of only one square inch. Joule also studied the effect of varying the strength of the current passing through the surrounding coil, and ascertained that while up to a certain point increase of current was accompanied by marked increase of lifting-power, yet when the current exceeded a more or less definite limit, further increase of it produced comparatively little effect. Reasoning upon his experiments, he formed the opinion—in which long afterwards Rowland concurred—that no current, however great, “could give an attractive power equal to 200 lbs. per square inch.”

It has, however, since been shown that this statement is not quite true. In the course of some experiments made in 1886, with a semi-

circular electro-magnet and a semicircular armature of soft iron, a weight of more than 200 lbs. per square inch was easily carried, though the current was very far indeed from being infinite; it was, in fact, about 5 amperes.

If, as we have seen to be the case, there is no limit to the number of magnetic lines which can be induced in an iron bar, then, theoretically, there can be no limit to the lifting-power which an electro-magnet can be made to exhibit. Practically, however, a limit is imposed by the fact that we cannot command an unlimited current of electricity, nor would wires of any known material convey it even if we could. With sufficient current a little 3-inch electro-magnet might no doubt be made to lift a weight of a ton, but any attempt to pass such a current would result in the immediate fusion, or even vaporisation, of the wire-coils by the intense heat that would be generated.

The lifting power of an electro-magnet with an iron armature is proportional to the square of the total number of magnetic lines which run through the iron, inclusive of those due simply to the current in the coil. Ewing's experiments enable us to determine the greatest weight that a magnetised iron bar could support by itself, without any assistance from the surrounding coil. In the case of his specimen of iron it would be about 260 lbs. per square inch of section.

The permeability of an iron rod depends not only upon the intensity of the field in which the rod is placed, but also to some extent upon the physical condition of the iron, and is affected by such causes as mechanical stress or changes of temperature. If, for instance, we hang an iron wire vertically in a not very strong field, and stretch it a little by attaching a weight to its lower end, we shall find that the stretching causes a temporary increase in the longitudinal permeability of the wire. But if the experiment be repeated in a strong field, the effect will be reversed; the same load which before increased the permeability of the wire will be found to diminish it. In a field of a certain medium strength which can be determined by trial, the stretching will have no effect at all upon the permeability. This value of the field is called, after the first discoverer of the phenomenon, the "Villari Critical Point" for a certain load.

The permeability of a nickel wire appears to be always diminished by stretching, whatever the strength of the field or the magnitude of the load.

As the magnetic qualities of a rod of iron or other magnetisable metal are affected by a temporary strain or slight alteration of its form, so it has been found that the form of such a rod may be slightly altered by magnetising it. By the aid of very delicate apparatus it is possible to show that in a continually increasing field the length of an iron bar is at first increased, and afterwards diminished; that of a cobalt bar is at first diminished, and afterwards increased; while that of a nickel bar is always diminished. The following table shows the

nature of the changes of length undergone by certain rods of iron, cobalt, and nickel, when magnetised.

Magnetising Force.	Elongations in ten-millionths of Original Length.		
	Iron.	Cobalt.	Nickel.
65	13	..	- 104
125	19*	- 10	- 167
237	7	- 31	- 218
293	0	- 37	- 233
343	- 6	- 44†	- 240
500	- 35	- 30	..
745	- 50	0	..
1120	- 65	45	..
1400	- 66	75	- 245

\* Maximum increment.

† Maximum decrement.

It was shown by Professor J. J. Thomson, a year or two ago, that the elongations and contractions of iron under magnetisation are intimately connected with the phenomenon which has been referred to as the Villari reversal. With a knowledge of the Villari effect, the elongation and subsequent contraction of an iron rod under magnetisation might have been predicted, and *vice versâ*. Now, since the elongations and contractions of cobalt are of the opposite character to those of iron, Professor Thomson's reasoning would lead us to expect a Villari effect in cobalt, which would also be of the opposite character. Quite recently, Mr. Chree, at Professor Thomson's suggestion, made some experiments to test the accuracy of this presumption, and found the Villari reversal which was anticipated. Again, the circumstance that nickel is always shortened by magnetisation, and never lengthened, indicates that there is no Villari reversal in that metal; and, in fact, though one has been looked for by Professor Ewing and others, it has never been found.

A few words in conclusion with regard to the effect of heat. Iron, when gradually made very hot, loses its magnetic susceptibility quite suddenly at a low red heat, and practically becomes a non-magnetisable metal. Pure nickel loses the greater part of its magnetic quality at a much lower temperature, perhaps about 300° C. Both metals again become magnetisable when cold. Dr. Hopkinson has lately discovered a very remarkable effect of heat upon the magnetic properties of an alloy of iron and nickel. If a bar or wire of this alloy be made red hot, and then allowed to cool, it is rendered permanently non-magnetic, although the metals of which it is composed are by themselves both strongly magnetic. But if this non-magnetic material be cooled to a temperature a little below the freezing point, and then again allowed to resume the ordinary temperature of the air, it will be found to have become almost as strongly magnetic as a

piece of steel, and it will continue to be magnetic until it is once more made red hot. This is one of the most remarkable discoveries in magnetism that has been made for many years. It revives the question first suggested by Faraday—whether any metal whatever may not possibly be rendered magnetisable by exposure to a sufficiently low temperature.\*

[S. B.]

---

\* The discourse was illustrated by about twenty experiments.

Friday, March 14, 1890.

WILLIAM CROOKES, Esq. F.R.S. Vice President, in the Chair.

Professor T. E. THORPE, Ph.D. F.R.S. M.R.I.

*The Glow of Phosphorus.*

THE word *phosphorus*—originally applied to any substance, solid or liquid, which had the property of shining in the dark—has gradually lost its generic sense, and is nowadays practically restricted, as a designation, to the waxlike inflammable substance which plays such an important part in the composition of an ordinary lucifer match. Phosphorus, indeed, is one of the most remarkable of the many remarkable substances known to the chemist. The curious method of its discovery; the universality of its distribution; its intimate connection with the phenomena of animal and vegetable life; its extraordinary physical properties and chemical activity; its abnormal molecular constitution; the Protean ease of its allotropic transformations—all combine to make up a history which abundantly justifies its old appellation of *phosphorus mirabilis*.

Godfrey Hankewitz, more than 150 years ago, wrote: "This phosphorus is a subject that occupies much the thoughts and fancies of some alchemists who work on microcosmical substances, and out of it they promise themselves golden mountains." Certainly no man of his time made more in the way of gold out of phosphorus than did Mr. Hankewitz, for, at his little shop in the Strand, he enjoyed for many years the monopoly of its sale, guarding his *Arcana* with all the jealousy of a modern manufacturer of the element.

Phosphorus, or, as it was then called, *nottiluca*, was first seen in this country in 1677. It was shown to Robert Boyle, who had already worked on phosphorescence in general, and who seems to have been specially struck with the remarkable peculiarity of a factitious body which could be made "to shine in the dark without having been before illumined by any lucid substance, and without being hot as to sense." In these respects the substance differed from all the *phosphori* hitherto known. The conditions which determine its glow were the subject of the earliest observations on phosphorus, and Boyle has left us a minute account of his work on this point. In the first place, he noticed that the substance was only luminous in presence of air. He accurately describes the nature of the light, and noticed that the water in which the phosphorus was partially immersed acquired a "strong and penetrant taste . . . and relished a little like vitriol." "On evaporation it would not shoot into crystals . . but coagulated into a substance like a gelly, or the whites of eggs, which would be easily melted by heat." On heating this "gelly" it gave off "flashes of fire

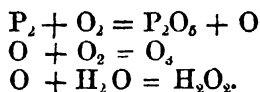
and light" and had a "garlick smell." He also found that the *noctiluca* was soluble in certain oils, and he particularly mentions oil of cloves as a convenient means of showing the luminosity, as it is "rendered more acceptable to the standers-by by its grateful smell." "In Oyl of Mace it did not appear luminous nor in Oyl of Aniseeds." Boyle describes a number of experiments showing how small a quantity of the phosphorus is required to produce a luminous effect. "A grain of the *noctiluca* dissolved in Alcohol of Wine and shaken in Water; it render'd 400,000 times its weight luminous throughout. And at another Tryal I found that it impregnated 500,000 times its weight; which was more than one part of Cochineel could communicate its colour to." "And one thing further observable was that when it had been a long time exposed to the air it emitted strong and odorous exhalations distinct from the visible Fumes." The strong and odorous exhalations we now know to be ozone.

The earlier volumes of the 'Philosophical Transactions' contain several papers on the luminosity of phosphorus, and one by Dr. Frederick Slare is noteworthy as giving one of the earliest, if not actually the earliest, account of what is one of the most paradoxical phenomena connected with the luminosity of phosphorus, namely, its increase on rarefying the air. "It being now generally agreed that the fire and flame [of phosphorus] have their pabulum out of the air, I was willing to try this matter *in vacuo*. To effect this, I placed a considerable lump of this matter [phosphorus] under a glass, which I fixed to an engine for exhausting the air; then presently working the engine, I found it grow lighter [i. e. more luminous], though a charcoal that was well kindled would be quite extinguished at the first exhaustion; and upon the third or fourth draught, which very well exhausted the glass, it much increased its light, and continued so to shine with its increased light for a long time; on re-admitting the air, it returns again to its former dulness." This observation was repeated, and its result confirmed by Hawksbee in this country, and by Homberg in France, and seems subsequently to have led Berzelius and after him Marchand, to the conclusion that the luminosity of phosphorus was altogether independent of the air (i. e. the oxygen), but was solely due to the volatility of the body. Many facts, however, combine to show that the air (oxygen) is necessary to the phenomenon. Lampadius found that phosphorus would not glow in the Torricellian vacuum, and Lavoisier, in 1777, showed that it would not inflame under the same conditions; and the subsequent experiments of Schrotter, Meissner, and Muller are decisive on the point that the glow is the concomitant of a chemical process dependent upon the presence of oxygen. It is, however, remarkable that phosphorus will not glow in oxygen at the ordinary atmospheric pressure and temperature, but that if the oxygen be rarefied the glow at once begins, but ceases again the moment the oxygen is compressed. Indeed, phosphorus will not glow in compressed air, and the flame of feebly-burning phosphorus may be extinguished by suddenly increasing the pressure

of the gas. Phosphorus, however, can be made to glow in oxygen at the ordinary pressure, or in compressed air, if the gases are gently warmed. In the case of oxygen the glow begins at  $25^{\circ}$ , and becomes very bright at  $36^{\circ}$ . In compressed air the temperature at which the glow is initiated depends upon the tension. If the oxygen is absolutely deprived of moisture, the phosphorus refuses to glow under any conditions. This fact, strange as it may seem, is not without analogy; the presence of traces of moisture appears to be necessary for the initiation or continuance of chemical combination in a number of instances.

It was observed by Boyle that a minute quantity of the vapour of a number of essential oils extinguished the glow of phosphorus. The late Professor Graham confirmed and extended these observations; he showed that relatively small quantities of olefiant gas, and of the vapours of ether, naphtha, and oil of turpentine entirely prevented the glow, and subsequent observers have found that many essential oils, such as those of peppermint and lemon, and the vapours of camphor and asafetida, even when present in very small quantity, stop the absorption of oxygen and the slow combustion of phosphorus in air.

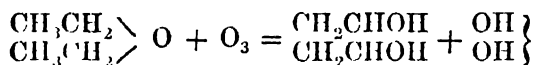
It has been established that whenever phosphorus glows in air, or in rarefied oxygen, ozone and hydrogen peroxide are formed, but it is not definitely known whether the formation of these substances is the cause or the effect of the chemical process of which the glow is the visible sign. That there is some intimate connection between the luminosity of the phosphorus and the production of these bodies is highly probable. Schonbein, as far back as 1848, sought to demonstrate that the glow depends on the presence of ozone. It is certainly true that many of the substances, such as the essential oils, which prevent the glow of phosphorus, also destroy ozone. At a low temperature phosphorus produces no ozone in contact with air, neither does it glow. It has been found, in fact, that with air ozone is produced in largest quantity at  $25^{\circ}$ , at which temperature phosphorus glows brightly. On the assumption that the oxidation of the phosphorus consists in the immediate formation of the highest oxide, the production of the ozone and the hydrogen peroxide has been represented by the following equations:—



Both these reactions may, of course, go on simultaneously, as ozone and hydrogen peroxide are not mutually incompatible; the synthesis of hydrogen peroxide by the direct oxidation of water seems to occur in a number of processes. But such symbolic expressions can at most be only very partial representations of what actually occurs. It is highly probable that the combination, which gives rise to the glow only occurs between the *vapour* of phosphorus and the oxygen. Phosphorus is sensibly volatile at ordinary temperatures, and by

rarefying the atmosphere in which it is placed its volatilisation is increased, which serves to account for the increased glow when the pressure of the gas is diminished. When phosphorus is placed in an atmosphere of hydrogen, nitrogen, or carbonic acid, these gases, when brought into contact with oxygen, become luminous from the oxidation of the vapour of phosphorus diffused through them. The rapidity of volatilisation varies with the particular gas; it is greatest in the case of hydrogen, and least in that of carbonic acid. Indeed, a stream of hydrogen gas at ordinary temperatures carries away comparatively large quantities of phosphorus, which may be collected by appropriate solvents. No ozone and no glow are produced in oxygen gas at ordinary temperatures and pressures, but on warming the oxygen both the ozone and the glow are formed. On passing ozone into oxygen at temperatures at which phosphorus refuses to glow, the phosphorus at once becomes luminous, oxygen is absorbed and the characteristic cloud of oxide is produced, and the effect continues so long as the supply of ozone is maintained. A drop of ether at once extinguishes the glow.

The ether is in all probability converted into vinyl alcohol with simultaneous formation of hydrogen peroxide by the reaction indicated by Poleck and Thummel



Formic, acetic, and oxalic acids are also formed by the action of ozonised oxygen on ether.

Phosphorus combines with oxygen in several proportions, and the study of the mode of formation and properties of these oxides is calculated to throw light upon the nature of the chemical process which attends the glow of phosphorus. Certain of these oxides have recently been the subject of study in the chemical laboratories of the Normal School of Science. When phosphorus is slowly burned in air, there is produced a considerable quantity of a volatile substance, having a characteristic garlic-like smell which solidifies, when cooled, in beautiful arborescent masses of white crystals. It melts at about 23°, and boils at 173°. In a sealed tube kept in the dark it may be preserved unchanged, but on exposure to light, and especially to bright sunshine, it rapidly becomes deep red. It slowly absorbs oxygen at the ordinary temperature and pressure, but from the mode in which the solid product of the reaction (P<sub>2</sub>O<sub>5</sub>) is deposited, it is evident that the union only takes place between the vapour of the oxide and the oxygen gas. Under diminished pressure the act of combination is attended with a glow which increases in brilliancy if ozone be present. On compressing the oxygen the glow ceases. No ozone is formed during the act of oxidation. The degree of rarefaction needed to initiate the glow depends upon the temperature of the oxide; the warmer the oxide the less is the diminution of pressure required. By gradually warming the oxide the luminosity steadily



increases both in area and intensity, until at a certain temperature the mass ignites. The change from glow to actual flame is perfectly regular and gradual, and is unattended with any sudden increase in brilliancy. In this respect the process of oxidation is analogous to the slow and barely visible burning of fire-damp which is sometimes seen to occur in the Davy lamp, or to the slow combustion of ether and other vapours which has been specially studied by Dr. Perkin. Other instances of what may be called *degraded combustion* are known to chemists. Thrown into warm oxygen the substance bursts into flame at once, and burns brilliantly; and it also takes fire in contact with chlorine. Alcohol also ignites it, and when it is warmed with water or a solution of potash it evolves spontaneously inflammable phosphorated hydrogen. In contact with cold water it suffers only a very gradual change, and many days may elapse before even a comparatively small quantity is dissolved. This substance has long been known; it was discovered, in fact, by the French chemist, Sage, but its true nature has only now been determined; its chemical formula is found to be  $P_4O_6$ ; hence its composition is similar to that of its chemical analogue arsenious oxide.

The study of the properties of this remarkable substance enables us to gain a clearer insight into the nature of the chemical change attending the glow of phosphorus. When phosphorus is placed in oxygen, or in an atmosphere containing oxygen under such conditions that it volatilises, the phosphorus oxidises, partly into phosphoric oxide, and partly into phosphorous oxide; ozone is formed, possibly in the mode already indicated, and this reacts upon the residual phosphorus vapour and the phosphorous oxide with the production of the luminous effect to which the element owes its name. The glow itself is nothing but a slowly burning flame having an extremely low temperature, caused by the chemical union of oxygen with the vapours of phosphorus and phosphorous oxide. By suitable means this glow can be gradually augmented, until it passes by regular gradation into the active vigorous combustion which we ordinarily associate with flame. Many substances, in fact, may be caused to phosphoresce in a similar way. Arsenic, when gently heated, glows in oxygen, and sulphur may also be observed to become luminous in that gas at a temperature of about  $200^{\circ}$ .

[T. E. T.]

Friday, March 21, 1890.

SIR JAMES CRICHTON BROWNE, M.D. LL.D. F.R.S. Treasurer and  
Vice-President, in the Chair.

PROFESSOR G. F. FITZ GERALD, M.A. F.R.S.

*Electromagnetic Radiation.*

IN order to discover whether actions are propagated in time or instantaneously, we may employ the principle of interference to measure the wave-length of a periodic disturbance, and determine whether it is finite or no. This is the principle employed by Hertz to prove experimentally Maxwell's theory as to the rate of propagation of electromagnetic waves. In order to confine the experiments within reasonable limits we require short waves, of a few metres' length at most. As the highest audible note gives waves of five or six miles long, and our eyes are sensitive only to unmanageably short waves, it is necessary to generate and observe waves whose frequency is intermediate between them, of some hundred million vibrations per second or so. For this purpose we may use a pair of conducting surfaces connected by a shorter or longer wire, in which is interposed a spark-gap of some few millimetres' length. When the conductors are charged by a coil or electrical machine to a sufficiently high difference of potential for a spark to be formed between them, they discharge in a series of oscillations, whose period for systems of similar shape is inversely proportional to the linear dimensions of the system so long as the surrounding medium is unaltered. When the surrounding non-conducting medium changes, the period depends on the electric and magnetic specific inductive capacities of this medium. Two such systems were shown. A large one, whose frequency was about 60 millions per second; and a small one, whose frequency was about 500 millions per second. The large one consisted of two flat plates, about 30 cm. square and 60 cm. apart, and arranged in the same way as is described by Prof. Hertz in Wiedemann's 'Annalen,' April 1888. The smaller vibrating system consisted of two short brass cylinders terminating in gilt brass balls of the same size, and arranged in the same way as the smaller system described by Prof. Hertz in Wiedemann's 'Annalen,' March 1889. This latter system was placed in the focal line of a cylindrical parabolic mirror of thin zinc plate, such as that described by Prof. Hertz in this paper.

These generators of electromagnetic oscillations may be called electric oscillators, as the electric charge oscillates from end to end. A circle of wire, or a coil in which an alternating current ran, or, if

such a thing were attainable, a magnet alternating in polarity, might be called a magnetic oscillator. A ring magnet with a closed magnetic circuit is essentially an electric oscillator, while a ring of ring magnets would be essentially a magnetic oscillator again. The elementary theory of a magnetic oscillator can be derived from that of an electric oscillator by simply interchanging electric and magnetic force. Electricity and magnetism would be essentially interchangeable if such a thing existed as magnetic conduction. The only magnetic currents we know are magnetic displacement currents and convection currents, such as are used in unipolar and some other dynamos. It is in this difference that we must look for the difference between electricity and magnetism.

In order to observe the existence of these electromagnetic oscillations we can employ the principle of resonance to generate oscillations in a system whose free period of oscillation is the same. A magnetic receiver may be employed consisting of a single incomplete circle of wire broken by a very minute spark-gap, across which a spark leaps when the oscillations in the wire become sufficiently intense. In order that a large audience may observe the occurrence of sparks, the terminals of a galvanometer circuit were connected one with one side of the spark-gap and the other with a fine point which could be approached very close to the other side of the spark-gap. It was observed that when a spark occurred in the gap, a spark could also be arranged to occur into the galvanometer circuit, and with a delicate long-coil galvanometer (that used had 40,000 ohms resistance) a very marked deflection can be produced whenever a spark occurs. This arrangement we have only succeeded in working comparatively close to the generator, because the delicacy required in adjusting the two spark-gaps is so great. It can, however, be employed to show that the sparks produced in this magnetic resonant circuit are due to resonance by removing this receiver from the generator to such a distance that sparks only just occur, and then substituting for the single circuit a double circuit, which, except for resonance, should have a greater action than the single one, but which stops the sparking altogether. An electric receiver was also used which was identical with the generator, and had a corresponding, only much smaller, spark-gap between the two plates. When the plates are connected with the terminals of the galvanometer, upon the occurrence of each spark the galvanometer is deflected. It is not so easy to obtain sparks when the plates are connected with the galvanometer as when they are insulated, and it is this that has limited the use of this method of observation. By making the first metro or so of the wires to the galvanometer of extremely fine wire, so as to reduce their capacity, we have found that the difficulty of getting sparks is less than with thick wires. We have not observed any effect due to the thickness of the wires after a short distance from the receiver.

In the case of the small oscillator, a receiver exactly like the one described by Prof. Hertz in his second paper already quoted was

placed in the focal line of a cylindrical parabolic mirror, and its receiving wires were connected with the wires leading to the galvanometer by some very fine brass wire. With the large sized generator and receiver, which were placed about 3 metres apart, it was shown that the sparking was stopped by placing a thin zinc sheet so as to reflect the radiations from a point close behind the receiver. By means of a long indiarubber tube hung from the ceiling it was shown how, when waves are propagated to a point whence they are reflected, the direct and reflected waves interfering produce a system of loops and nodes, with a node at the reflecting point. It was explained that these nodes, though places of zero displacement, were places of maximum rotation, and that the axis of rotation was at right angles to the direction of displacement. It was explained that an analogous state of affairs existed in the electromagnetic vibrations. If the electric force be taken as analogous to the displacement of the rope, the magnetic may be taken as analogous to its rotation, and the two are at right angles to one another. In the ether the electric node is a magnetic loop, and *vice versa*. Though the two are separated in loops and nodes, they exist simultaneously in a simple wave propagation, just as in a rope when propagating waves in one direction the crest of maximum displacement is also that of maximum rotation. It was explained that by placing the reflector at a quarter of a wavelength from the receiver this would be at an electric loop, and have its sparking increased. It may thus be shown that there are a series of loops and nodes produced by reflection of these electromagnetic forces, like those produced in any other case of reflected wave-propagation. This was Hertz's fundamental experiment, by which he proved that electromagnetic actions are propagated in time, and by some approximate calculations he verified Maxwell's theory that the rate of propagation is the same as that of light. It follows that the luminiferous ether is experimentally shown to be the medium to which electric and magnetic actions are due, and that the electromagnetic waves we have been studying are really only very long light waves.

A rather interesting deduction from Maxwell's theory is that light incident on any body that absorbs or reflects it should press upon it and tend to move it away from the source of light. Illustrating this, an experiment was shown with an alternating current passing through an electro-magnet, in front of which a good conducting plate of silver was suspended. When the alternating current was turned on the silver was repelled. It was explained that as the silver could only be affected by what was going on in its own neighbourhood, and that if sufficiently powerful radiations from a distant source were falling on the silver, it would be acted on by alternating magnetic forces, this experiment was in effect an experiment on the repulsion of light, which was too small to have been yet observed, even in the case of concentrated sunshine. These slow vibrations are not stopped by a sheet of zinc, though much reduced by a magnetic sheet like tin-



plate, though the rapid ones are quite stopped by either—thus showing that wave-propagation in a conductor is of the nature of a diffusion.

In all cases of diffusion where we consider the limits of the problem, terms involving the momentum of the parts of the body must be introduced. It appears from elementary theories of diffusion as if it were propagated instantaneously, but no action can be propagated from molecule to molecule, in air, for instance, faster than the molecules move, i. e. at a rate comparable with that of sound. In electromagnetic theory corresponding terms come in by introducing displacement currents in conductors, and it seems impossible but that some such terms should be introduced, as otherwise electromagnetic action would be propagated instantaneously in conductors. The propagation of light through electrolytes, and the too great transparency of gold leaf, point in the same direction.

The constitution of these waves was then considered, and it was explained that if magnetic forces are analogous to the rotation of the elements of a wave, then an ordinary solid cannot be analogous to the ether because the latter may have a constant magnetic force existing in it for any length of time, while an elastic solid cannot have continuous motion of its elements in one direction existing within it. The most satisfactory model, with properties quite analogous to those of the ether is one consisting of wheels geared with elastic bands. The wheels can rotate continuously in one direction, and their rotation is the analogue of magnetic force. The elastic bands are stretched by a difference of rotation of the wheels, and introduce stresses quite analogous to electric forces. By making the elastic bands of lines of governor balls, the whole model may have only kinetic energy, and so represent a fundamental theory. Such a model can represent media differing in electric and magnetic inductive capacity. If the elasticity of the bands be less in one region than another, such a region represents a body of higher electric inductive capacity, and waves would be propagated more slowly in it. A region in which the masses of the wheels was large would be one of high magnetic inductive capacity. A region where the bands slipped would be a conducting region. Such a model unlike most others proposed, illustrates both electric and magnetic forces and their inter-relations, and consequently light propagation.

In the neighbourhood of an electric generator the general distribution of the electric and magnetic forces is easily seen. The electric lines of force must lie in planes passing through the axis of the generator, while the lines of magnetic force lie in circles round this axis and perpendicular to the lines of electric force. It is thus evident that the wave is, at least originally, polarised. To show this, the small-sized oscillators with parabolic mirrors were used, and a light square frame, on which wires parallel to one direction were strung, was interposed between the mirrors. It was shown that such a system of wires was opaque to the radiation when the wires were parallel to the electric force, but was quite transparent when the

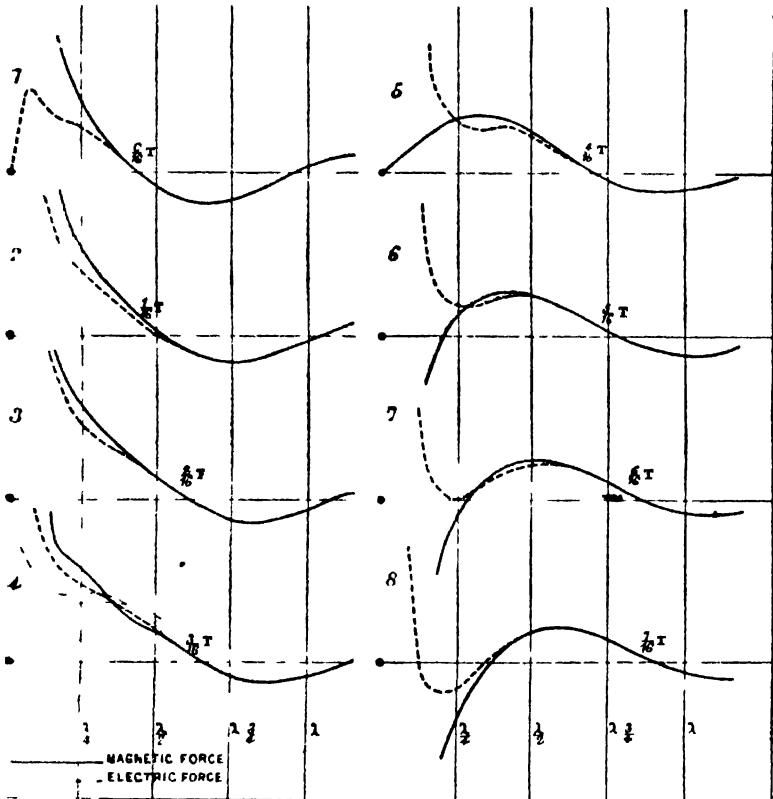
frame was turned so that the wires were parallel to the magnetic force. It behaved just like a tourmaline to polarised light. It is of great interest to verify experimentally Maxwell's theory that the plane of polarisation of light is the plane of the magnetic force. This has been done by Mr. Trouton, who has shown that these radiations are not reflected at the polarising angle by the surface of a non-conductor, when the plane of the magnetic force in the incident vibration is perpendicular to the plane of incidence, but the radiations are reflected at all angles of incidence when the plane of the magnetic force coincides with the plane of incidence. Thus the long-standing dispute as to the direction of vibration of light in a polarised ray has been at last experimentally determined. The electric and magnetic forces are not simultaneous near the oscillator. The electric force is greatest when the electrification is greatest, and the magnetic force when the current is greatest, which occurs when the electrification is zero: thus the two, when near the oscillator, differ in phase by a quarter of a period. In the waves, as existing far from the oscillator, they are always in the same phase. It is interesting to see how one gains on the other. It may be worth observing again that though what follows deals with electric oscillators, the theory of magnetic oscillators is just the same, only that the distribution of magnetic and electric forces must be interchanged. Diagrams drawn from Hertz's figures published in Wiedemann's 'Annalen' for January 1889, and in 'Nature' for March 7th, 1889, and in the 'Philosophical Magazine' for March 1890, were thrown on the screen in succession, and it was pointed out how the electric wave, which might be likened to a diverging whirl ring, was generated, not at the oscillator, but at a point about a quarter of a wave length on each side of the oscillator, while it was explained that the magnetic force wave starts from the oscillator. It thus appears how one gains the quarter period on the other. The outflow of the waves was exhibited by causing the images to succeed one another rapidly by means of a zoetrope, in which all the light is used and the succession of images formed by having a separate lens for each picture and rotating the beam of light so as to illuminate the pictures in rapid succession.

As the direction of flow of energy in an electromagnetic field depends on the directions of electric and magnetic force, being reversed when either of these is reversed, it follows that in the neighbourhood of the oscillator the energy of the field alternates between the electric and magnetic forms, and that it is only the energy beyond about a quarter of the wave length from the oscillator which is wholly radiated away during each vibration. It follows that in ordinary electromagnetic alternating currents at from 100 to 200 alternations per second, it is only the energy which is some 3000 miles away which is lost. If an electromagnetic wave, having magnetic force comparable to that near an ordinary electro-magnet, were producible, the power of the radiation would be stupendous. If we consider the possible radiating power of an atom by calculating it upon

the hypothesis that the atomic charge oscillates across the diameter of the atom, we find that it may be millions of millions of times as great as Prof. Wiedemann has found to be the radiating power of a sodium atom in a Bunsen burner, so that if there is reason to think that any greater oscillation might disintegrate the atom, it is evident that we are still a long way from doing so. It is to be observed that ordinary light waves are very much longer than the period of the vibration above referred to. Dr. Lodge has pointed out that quite large oscillators in comparison to molecules, namely, about the size of the rods and cones in the retina, are of the size to resound to light waves of the length we see, and so might be used to generate such waves. This seems to show that the electro-magnetic structure of an atom must be more complicated than a small sphere or other simple shape with an oscillating charge on it, for the period of vibration of a small system can be made long by making the system complex, e. g. a small Leyden jar of large capacity with a long wire wound many times round connecting its coats, could easily be constructed to produce electromagnetic waves whose length would bear the same proportion to the size of the jar as ordinary light waves do to an atom. The rate at which the energy of a Hertzian vibrator is transferred to the ether is so great that we should expect an atom to possess the great radiating power it has. This shows, on the other hand, how completely the vibrations of an atom must be forced by the vibrations of the ether in its neighbourhood, so that atoms, being close compared with a wave length, are, in any given small space, probably in similar phases of vibration. It is interesting to consider this in connection with the action of molecules in collision as to how far the forces between molecules after collision is the same as before. In the same connection the existence of intra-atomic electromagnetic oscillations is interesting in the theories of anomalous dispersion. An electromagnetic model of a prism with anomalous dispersion might be constructed out of pitch, through which conductors, each with the same rate of electromagnetic oscillation, were dispersed. In theories of dispersion a dissipation of energy is assumed, and it may be the radiation of the induced electromagnetic vibrations. These can evidently never be greater than the incident electromagnetic vibration, on account of this radiation of their own energy. In some theories a vibration of something much less than the whole molecule is assumed, and the possibility of intra-atomic electromagnetic oscillations would account for this. Some such assumption seems also required, in order to explain such secondary, if not tertiary, actions as the Hall effect and the rotation of the plane of polarisation of light, which are, apparently at least, secondary actions due to a reaction of the matter set in motion by the radiation on this radiation.

Some further diagrams were exhibited, plotted from Hertz's theory by Mr. Trouton, to whom much of the matter in this paper is due. They are here reproduced, and show eight simultaneous positions of the electric and magnetic waves during a semi-oscillation

of an electric oscillator. The dotted line shows the electric force at various points, and the continuous line the magnetic force. In the first diagram the magnetic force is at its maximum near the origin, while the electric force there is zero. In the second the magnetic energy near the origin has partly turned into electric energy, and consequently electric force begins. The succeeding figures show how the magnetic force decreases near the origin, while the electric force grows and the waves already thrown off spread away. The change of magnetic force between Figures 4 and 5 is so rapid,



that a few dashed lines, showing interpolated positions, are introduced to show how it proceeds. It will be observed how a hollow comes in the line showing electric force, which gradually increases, and, crossing the line of zero force at about a quarter of a wave-length from the origin, is the source of the electric wave, which, starting with this odds, picks up and remains thenceforward coincident with the magnetic wave. From this origin of electric waves they spread out along with the magnetic waves and in towards the origin, to be reproduced again from this point on the next vibration. These electric and



magnetic forces here shown as coincident are, of course, in space in directions at right angles to one another as already explained. The corresponding diagrams for a magnetic oscillator are got by interchanging the electric and magnetic forces.

A further experiment was shown to illustrate how waves of transverse vibration can be propagated along a straight hollow vortex in water. It was stated that what seemed a possible theory of ether and matter was that space was full of such infinite vortices in every direction, and that among them closed vortex rings represented matter threading its way through the ether. This hypothesis explains the differences in Nature as differences of motion. If it be true, ether, matter, gold, air, wood, brains are but different motions. Where alone we can know what motion in itself is, that is, in our own brains, we *know* nothing but thought. Can we resist the conclusion that all motion is thought? Not that contradiction in terms, unconscious thought, but living thought; that all Nature is the language of One in whom we live, and move, and have our being.

[G. F. F. G.]

Friday, March 28, 1890.

SIR FREDERICK BRAMWELL, Bart. D.C.L. F.R.S. Honorary Secretary  
and Vice-President, in the Chair.

The Right Hon. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*  
Professor of Natural Philosophy, R.I.

*Foam.*

WHEN I was turning over in my mind the subject for this evening, it occurred to me to take as the title of the lecture, "Froth." But I was told that a much more poetical title would be "Foam," as it would so easily lend itself to appropriate quotations. I am afraid, however, that I shall not be able to keep up the poetical aspect of the subject very long; for one of the things that I shall have most to insist upon is that foaming liquids are essentially impure, contaminated—in fact, dirty. Pure liquids will not foam. If I take a bottle of water and shake it up, I shall get no appreciable foam. If, again, I take pure alcohol, I get no foam. But if I take a mixture of water with 5 per cent. of alcohol there is a much greater tendency. Some of the liquids we are most familiar with as foaming, such as beer or ginger-beer, owe the conspicuousness of the property to the development of gas in the interior, enabling the foaming property to manifest itself; but of course the two things are quite distinct. Dr. Gladstone proved this many years ago by showing that beer from which all the carbonic acid had been extracted in *vacuo* still foamed on shaking up. I now take another not quite pure but strong liquid, acetic acid, and from it we shall get no more foam than we did from the alcohol or the water. The bubbles, as you see, break up instantaneously. But if I take a weaker acid, the ordinary acid of commerce, there is more, though still not much, tendency to foam. But with a liquid which for many purposes may be said to contain practically no acetic acid at all, seeing that it consists of water with but 1-1000th part of acid, the tendency is far stronger; and we get a very perceptible amount of foam. These tests with the alcohol and acetic acid are sufficient to illustrate the principle that the property of foaming depends on contamination. In pure ether we have a liquid from which the bubbles break even more quickly than from alcohol or water. They are gone in a moment. In some experiments I made at home I found that water containing a small proportion of ether foamed freely; but on attempting two or three days ago to repeat the experiment, I was surprised to

find a result very different. I have here some water containing a very small fraction of ether, about 1-240th part. If I shake it up, it scarcely foams at all; but another mixture made in the same proportion from another sample shows more tendency to foam. This is rather curious, because both ethers were supposed to be of the same quality; but one had been in the laboratory longer than the other, and perhaps contained more greasy matter in solution.

Another liquid which foams freely is water impregnated with camphor. Camphor dissolves sparingly; but a minute quantity of it quite alters the characteristics of water in this respect. Another substance, very minute quantities of which communicate the foaming property to water, is glue or gelatine. This liquid contains only 3 parts in 100,000 of gelatine, but it gives a froth entirely different from that of pure water. Not only are there more bubbles, but the duration of the larger bubbles is quite out of proportion to that of water-bubbles. This sample contains 5 parts in 100,000, nearly double as much; but even with but 1 part in 100,000, the foaming property is so evident as to suggest that it might in certain cases prove valuable for indicating the presence of minute quantities of impurities. I have been speaking hitherto of those things which foam slightly. They are not to be compared with, say, a solution of soap in water, which, as is well known to everybody, froths very vigorously. Another thing comparable to soap, but not so well known, is saponine. It may be prepared from horse chestnuts by simply cutting them in small slices and making an infusion with water. A small quantity of this infusion added to water makes it foam strongly. The quantity required to do this is even less than in the case of soap; so the test is more delicate. It is well known that rivers often foam freely. That is no doubt due to the effect of saponine or some analogous substance. Sea-water foams, but not, I believe, on account of the saline matter it contains; for I have found that even a strong solution of pure salt does not foam much. I believe it has been shown that the foaming of sea-water, often so conspicuous, is due to something extracted from seaweeds during the concussion which takes place under the action of breakers.

Now let us consider for a moment what is the meaning of foaming. A liquid foams when its films have a certain durability. Even in the case of pure water, alcohol, and ether, these films exist. If a bubble rises, it is covered for a moment by a thin film of the liquid. This leads us to consider the properties of liquid films in general. One of their most important and striking properties is their tendency to contract. Such surfaces may be regarded as being in the condition of a stretched membrane, as of india-rubber, only with this difference, that the tendency to contract never ceases. We may show that by blowing a small soap bubble, and then removing the mouth. The air is forced back again by the pressure exerted on the bubble by the tension of the liquid. This ancient experiment suffices to prove conclusively that liquid films exercise tension.

A prettier form of the same experiment is due to Van der Mensbrugghe, who illustrated liquid tension by means of a film in which he allowed to float a loop of fine silk, tied in a knot. As long as the interior of the loop, as well as the exterior, is occupied by the liquid film, it shows no tendency to take any particular shape: but if, by insertion of, say, a bit of blotting paper, the film within the loop be ruptured, then the tension of the exterior film is free to act, and the thread flies instantaneously into the form of a circle, in consequence of the tendency of the exterior surface to become as small as possible. The exterior part is now occupied by the soap film, and the interior is empty. Many other illustrations of this property of liquids might be given, but time does not permit.

In the soap film, as in the films which constitute ordinary foam, each thin layer of liquid has two surfaces; each tends to contract; but in many cases we have only one such surface to consider, as when a drop of rain falls through the air. Again, suppose that we have three materials in contact with one another,—water, oil, and air. There are three kinds of surfaces separating the three materials, one separating water and oil, another oil and air, and a third surface separating the water from the air. These three surfaces all exert a tension, and the shape of the mass of oil depends upon the relative magnitudes of the tensions. As I have drawn it here (Fig. 1), it is implied that the tension of the water-air surface is less than the sum of the other two tensions—those of the water-oil surface and the air-oil surface; because the two latter acting obliquely balance the former. It is only under such conditions that the equilibrium of the three materials as there drawn in contact with one another is possible. If the tension of the surface separating water and air exceeded the sum of the other two, then the equilibrium as depicted would be impossible. The water-air tension, being greater, would assert its superiority by drawing out the edge of the lens, and the oil would tend to spread itself more and more over the surface.

FIG. 1.



And that is what really happens. Accurate measurements made by Quincke and others, show that the surface tension separating water and air, is really greater than the sum of the two others. So oil does tend to spread upon a surface of water and air. That this is the fact, we can prove by a simple experiment. At the feet of our chairman, our Honorary Secretary, is a large dish, containing water which at present is tolerably clean. In order to see what may happen to the surface of the water, it is dusted over with fine sulphur powder, and illuminated with the electric light. If I place on the surface

a drop of water, no effect ensues; but if I take a little oil, or better still a drop of saponine, or of soap-water, and allow that to be deposited upon the middle of the surface, we shall see a great difference. The surface suddenly becomes dark, the whole of the dust being swept away to the boundary. That is the result of the spread of the film, due to the presence of the oil.

How then is it possible that we should get a lens-shaped mass of oil, as we often do, floating upon the surface of water? Seeing that the general tendency of oil is to spread over the surface of water, why does it not do so in this case? The answer is that it has already spread, and that this surface is not really a pure water surface at all, but one contaminated with oil. It is in fact only after such contamination that an equilibrium of this kind is possible. The volume of oil necessary to contaminate the surface of the water is very small, as we shall see presently; but I want to emphasise the point that, so far as we know, the equilibrium of the three surfaces in contact with one another is not possible under any other conditions. That is a fact not generally recognised. In many books you will find descriptions of three bodies in contact, and a statement of the law of the angles at which they meet; that the sides of a triangle, drawn parallel to the three intersecting surfaces must be in proportion to the three tensions. No such equilibrium, and no such triangle, is possible if the materials are pure; when it occurs, it can only be due to the contamination of one of the surfaces. These very thin films, which spread on water, and, with less freedom, on solids also, are of extreme tenuity; and their existence alongside of the lens, proves that the water prefers the thin film of oil to one of greater thickness. If the oil were spread out thickly, it would tend to gather itself back into drops, leaving over the surface of the water a film of less thickness than the molecular range.

One experiment by which we may illustrate some of these effects I owe to my colleague, Professor Dewar. It shows the variation in the surface tension of water, due to the presence on it of small quantities of ether. I hold in my hand masses of charcoal, which can be impregnated with ether. The greater part of the surface of the charcoal is covered with paraffin wax, and, in consequence, the ether which has already penetrated the charcoal can only escape from it again on one side. The result is that the water in the rear of this boat of charcoal will be more impregnated with ether than the part in front, so the mass of charcoal will enter into motion, and the motion will extend over a considerable interval of time. As long as the ether remains in sufficient quantity to contaminate the water in the rear, so long is there a tendency to movement of the mass. The water covered with the film of ether has less tension than the pure water in front, and the balance of tensions being upset, the mass is put in motion. If the nature of the case is such that the whole surface surrounding the solid body is contaminated, then there is no tendency to movement, the same balance in fact obtaining as if the water were pure.

Another body which we may use for this purpose is camphor. If we spread some camphor scrapings on a surface of pure water, they will, if the surface is quite clean, enter into vigorous movement, as you now see. This is because the dissolved camphor diminishes the surface tension of the water. But if I now contaminate the water with the least possible quantity of grease, the movements of the camphor will be stopped. I merely put my finger in, and you observe the effect. There is not much poetry about that! A very slight film, perfectly invisible by ordinary means, is sufficient so to contaminate the water that the effect of the dissolved camphor is no longer visible.

I was very desirous to ascertain, if possible, the actual thickness of oil necessary to produce this effect, because all data relating to molecules are, in the present state of science, of great interest. From what I have already said, you may imagine that the quantity of oil required is very small, and that its determination may be difficult. In my experiments,\* I used the surface of water contained in a large sponge bath three feet in diameter. By this extension of the surface, I was able to bring the quantity of oil required within the range of a sensitive balance. In Diagram 2, I have given a number of results

#### DIAGRAM 2.

##### A SAMPLE OF OIL SOMEWHAT DECOLOURED BY EXPOSURE.

Date	Weight of Oil.	Calculated Thickness of Film in Micro-millimetres	Effect upon Camphor Fragments
Dec. 17 ..	mg 0.40	0.81	No distinct effect.
Jan. 11 ..	0.52	1.06	Barely perceptible.
Jan. 14 ..	0.65	1.32	Not quite enough
Dec. 20 ..	0.78	1.58	Nearly enough.
Jan. 11 ..	0.78	1.58	Just enough
Dec. 17 ..	0.81	1.63	Just about enough.
Dec. 18 ..	0.83	1.68	Nearly enough.
Jan. 22 ..	0.84	1.70	About enough.
Dec. 18 ..	0.95	1.92	Just enough.
Dec. 17 ..	0.99	2.00	All movements very nearly stopped.
Dec. 20 ..	1.31	2.65	Fully enough.

##### A FRESH SAMPLE.

Jan. 28 ..	0.63	1.28	Barely perceptible.
Jan. 28 ..	1.06	2.14	Just enough

obtained at various dates, showing the quantity of oil required to produce the effects recorded in the fourth column. Knowing the weight of the oil deposit, and the area of the water surface upon which it was uniformly spread, it was easy to calculate the thickness of the film.

\* Proc. Roy. Soc., March 1890.

It is seen that a film of oil about  $1\frac{1}{2}$  millionth of a millimetre thick is able to produce this change. I know that large numbers are not readily appreciated, and I will therefore put the matter differently. The thickness of the oil film thus determined as sufficient to stop the motions of the camphor is one 400th of the wave length of yellow light. Another way of saying the same thing is that this thickness of oil bears to one inch the same ratio that one second of time bears to half a year.

When the movement of the camphor has been stopped by the addition of a minute quantity of oil, it is possible, by extending the water surface enclosed within the boundary, without increasing the quantity of oil, to revive the movements of the camphor; or, again, by contraction, to stop them. I can do this with the aid of a flexible boundary of thin sheet brass, and you see that the camphor recovers its activity, though a moment ago it was quite dead. It would be an interesting subject for investigation to determine what is the actual tension of an oily surface contaminated to an extent just sufficient to stop the camphor movements; but it is not an easy problem. Usually we determine surface tensions by the height to which the liquids will rise in very fine tubes. Here, however, that method is not available, because if we introduce a tube into such a surface, there is no proof that the contamination of the inner surface in the tube is the same as that prevailing outside. Another method, however, may be employed which is less open to the above objection, and that is to substitute for the very fine or capillary tube, a combination of two parallel plates open at their edges. We have here two such plates of glass, kept from absolutely closing by four pieces of thin metal inserted at the corners, the plates being held close against these distance-pieces by suitable clamps. If such a combination be inserted in water, the liquid will rise above the external level, and the amount of the rise is a measure of the surface tension of the water. You see now the image on the screen. A is the external water surface; B is the height of the liquid contained between the glass plates, so that the tension may be said to be measured by the distance AB. If a little oil be now deposited upon the surface, it will find its way between the plates. The fall which you now see shows that the surface tension has been diminished by the oil which has found its way in. A very minute quantity will give a great effect. When the height of the pure water was measured by 62, a small quantity of oil changed the 62 into 48, and subsequent large additions of oil could only lower it to 38. But after oil has done its worst, a further effect may be produced by the addition of soap. If Mr. Gordon now adds some soap, we shall find that there is a still further fall in the level, showing that the whole tension now in operation is not much more than one-third of what it was at first. This is an important point, because it is sometimes supposed that the effect of soap in diminishing the tension of water is due to merely the formation upon the surface of a layer of oil formed by decomposition of the soap. This experiment proves the

contrary, because we find that soap can do so much more than oil. There is indeed, something more or less corresponding to the decomposition of the soap and the formation of a superficial layer of oil. But the decomposition takes place in a very peculiar manner, and under such conditions that there is a gradual transition from the soapy liquid in the interior to the oily layer at the top, and not, as when we float a layer of oil on water, two sudden transitions, first from water to oil, and secondly from oil to air. The difference is important, because, as I showed some years ago, capillary tension depends on the suddenness of change. If we suppose that the change from one liquid to another takes place by slow stages, though the final change may be as before, the capillary tension would absolutely disappear.

There is another very interesting class of phenomena due to oil films, which I hope to illustrate, though I am conscious of the difficulty of the task,—namely, the action of oil in preventing the formation of waves. From the earliest times we have records of the effect of oil in stilling waves, and all through the Middle Ages the effect was recognised, though connected with magic and fanciful explanations. Franklin, than whom, I suppose, no soberer inquirer ever existed, made the thing almost a hobby. His attention was called to it accidentally on board ship from noticing the effect on the waves caused by the greasy *débris* of a dinner. The captain assured him that it was due to the oil spread on the water, and for some time afterwards, Franklin used to carry oil about with him, so as never to miss a chance of trying an experiment. A pond is necessary to illustrate the phenomena properly, but we shall get an idea of it by means of this trough six feet long, containing water.\* Along the surface of the water we shall make an artificial wind by means of a fan,† driven by an electro motor. In my first experiments I used wind from an organ bellows, which is not here available. Presently we shall get up a ripple, and then we will try the effect of a drop of oil put in to windward. I have now put on the drop, and you see a smooth place advancing along. As soon as the waves come up again, I will repeat the experiment. While the wind is driving the oil away, I may mention that this matter has been tested at Peterhead. Experiments were there made on a large scale to show the effect of oil in facilitating the entrance of ships into harbour in rough weather. Much advantage was gained. But here a distinction must be observed. It is not that the large swell of the ocean is damped down. That would be impossible. The action in the first instance is upon the comparatively small ripples. The large waves are not directly affected by the oil; but it seems as if the power of the wind to excite and maintain them is due to the small ripples which form on their backs,

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\* The width is 8 inches, and the depth 4 inches. The sides are of glass; the bottom and ends of wood, painted white.

† For this fan and its fittings the Institution is indebted to the liberality of the Blackman Ventilating Company.



and give the wind, as it were, a better hold of them. It is only in that way that large waves can be affected. The immediate effect is on the small waves which conduce to that breaking of the large waves which from the sailor's point of view is the worst danger. It is the breaking waters which do the mischief; and these are quieted by the action of the oil.

I want to show also, though it can only be seen by those near, the return of the oil when the wind is stopped. The oil is at present driven to one end of the trough; \* when the wind stops it will come back, because the oil film tends to spread itself uniformly over the surface. As it comes back, there will be an advancing wave of oil; and as we light the surface very obliquely by the electric lamp, there is visible on the bottom of the trough a white line, showing its progress.

Now, as to the explanation. The first attempt on the right lines was made by the Italian physicist, Marangoni. He drew attention to the importance of contamination upon the surface of the water, and to its tendency to spread itself uniformly, but for some reason which I cannot understand, he applied the explanation wrongly. More recently Reynolds and Aitken have applied the same considerations with better success. The state of the case seems to be this:—Let us consider small waves as propagated over the surface of clean water; as the waves advance, the surface of the water has to submit to periodic extensions and contractions. At the crest of a wave the surface is compressed, while at the trough it is extended. As long as the water is pure there is no force to oppose that, and the wave can be propagated without difficulty; but if the surface be contaminated, the contamination strongly resists the alternate stretching and contraction. It tends always, on the contrary, to spread itself uniformly; and the result is that the water refuses to lend itself to the motion which is required of it. The film of oil may be compared to an inextensible membrane floating on the surface of the water, and hampering its motion; and under these conditions it is not possible for the waves to be generated, unless the forces are very much greater than usual. That is the explanation of the effect of oil in preventing the formation of waves.

The all-important fact is that the surface has its properties changed, so that it refuses to submit to the necessary extensions and contractions. We may illustrate this very simply by dusting the surface of water with sulphur powder, only instead of dispersing the sulphur, as before, by the addition of a drop of oil, we will operate upon it by a gentle stream of wind projected downwards on the surface, and of course spreading out radially from the point of impact. If Mr. Gordon will blow gently on the surface in the middle of the dusty region, a space is cleared; † if he stops blowing, the dust comes back

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\* May 1890. Any moderate quantity of oil may be driven off to leeward; but if oleate of soda be applied, the quieting effect is permanent.

† This experiment is due to Mr. Aitken

again. The first result is not surprising, but why does the dusty surface come back? Such return is opposed to what we should expect from any kind of viscosity, and proves that there must be some force directly tending to produce that particular motion. It is the superior tension of the clean surface. No oil has been added here, but then no water surface is ever wholly free from contamination; there may be differences of degree, but contamination is always present to some extent. I now make the surface more dirty and greasy by contact of the finger, and the experiment no longer succeeds, because the jet of wind is not powerful enough to cleanse the place on which it impinges; the dirty surface refuses to go away, or if it goes in one direction it comes back in another.

I want now to bring to your notice certain properties of soap solutions, which, however, are not quite so novel as I thought when I first came upon them in my own inquiries.\* If we measure by statical or slow methods the surface tension of soapy water, we find that it is very much less than that of clean water. We can prove this in a very direct manner by means of capillary tubes. Here, shown upon the screen, are two tubes of the same diameter, in which, therefore, if the liquids were the same, there would be the same elevation; one tube dips into clean water, and the other into soapy water, and the clean water rises much (nearly three times) higher than the soapy water.

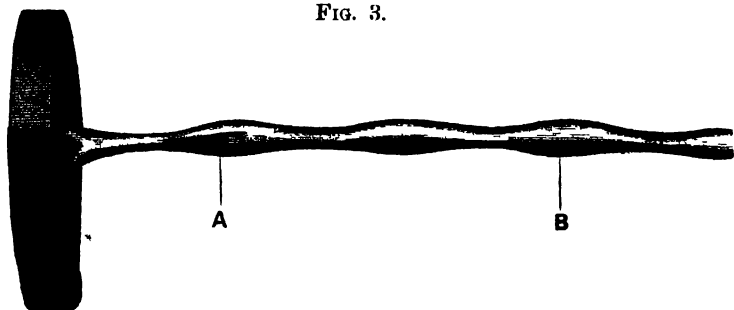
Although the tension of soapy water is so much less than that of pure water when measured in this way, I had some reason to suspect that the case might be quite different if we measured the tensions immediately after the formation of the surfaces. I was led to think so by pondering on Marangoni's view that the behaviour of foaming liquids was due to the formation of a pellicle upon their surfaces; for if the change of property is due to the formation of a pellicle, it is reasonable to suppose that it will take time, so that if we can make an observation before the surface is more than say  $\frac{1}{100}$  of a second old, we may expect to get a different result. That may seem an impossible feat, but there is really no difficulty about it; all that is necessary is to observe a jet of the substance in question issuing from a fine orifice. If such a jet issues from a circular orifice it will be cylindrical at first, and afterwards resolve itself into drops. If, however, the orifice is not circular, but elongated or elliptical, the jet undergoes a remarkable transformation before losing its integrity. As it issues from the elliptical orifice, it is in vibration, and trying to recover the circular form; it does so, but afterwards the inertia tends to carry it over to the other side of equilibrium. The section oscillates between the ellipse in one direction and the ellipse in the perpendicular direction. The jet thus acquires a sort of chain-like appearance, and the period of the movement represented by the

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\* I here allude to the experiments of Dupré, and to the masterly theoretical discussion of liquid films by Professor Willard Gibbs.

distance between corresponding points A, B, Fig. 3, is a measure of the capillary tension to which these vibrations of the elliptical section about the circular form are due. A measure, then, of the wave-length of the recurrent pattern formed by the liquid gives us information as to the tension immediately after escape; and if we wish to compare the tensions of various liquids, all we have to do is to fill a vessel alternately with one liquid and another, and compare the wave-lengths in the various cases. The jet issues from a flask, to which is attached below a tubular prolongation; the aperture is made small in order that we may be able to deal with small quantities of liquid. You now see the jet upon the screen (Fig. 3), it issues from the

FIG. 3.



orifice; it oscillates, and we can get a comparative measure of the tension by observing the distance between corresponding points (A, B).

If we were now to take out the water, and substitute for it a moderately strong solution of soap or saponine, we should find but little difference, showing that in the first moments the tension of soapy water is not very different from that of pure water. It will be more interesting to exhibit a case in which a change occurs. I therefore introduce another liquid, water containing 10 per cent. of alcohol, and you see that the wave-length is different from before. So this method gives us a means of investigating the tensions of surfaces immediately after their formation. If we calculate by known methods how long the surface has been formed before it gets to the point B, at which the measurement is concluded, we shall find that it does not exceed  $\frac{1}{100}$  of a second.

Another important property of contaminated surfaces is what Plateau and others have described as superficial viscosity. There are cases in which the surfaces of liquids—of distilled water, for example—seem to exhibit a special viscosity, quite distinct from the ordinary interior viscosity, which is the predominant factor in determining the rate of flow through long narrow tubes. Plateau's experiment was to immerse a magnetised compass needle in water; the needle turns, as usual, upon a point, and the water is contained in a cylindrical vessel, not much larger than the free rotation of the

needle requires (Fig. 4). The observation relates to the time occupied by the needle in returning to its position of equilibrium in the meridian, after having been deflected into the east and west positions, and Plateau found that in the case of water more time was required when the needle was just afloat than when it was wholly immersed, whereas in the case of alcohol the time was greater in the interior. The longer time occupied when the needle is upon the surface of water is attributed by Plateau to an excessive superficial viscosity of that body.

Instead of a needle, I have here a ring of brass wire (Fig. 5), floating on the surface of the water. You see upon the screen the image of the ring, as well as the surface of the water, which has been made visible by sulphur. The ring is so hung from a silk fibre that it can turn upon itself, remaining all the while upon the surface of the water. Attached to it is a magnetic needle, for the purpose of giving it a definite set, and of rotating it as required by an external magnet. On this water, which is tolerably clean, when the ring is made to

FIG. 4.

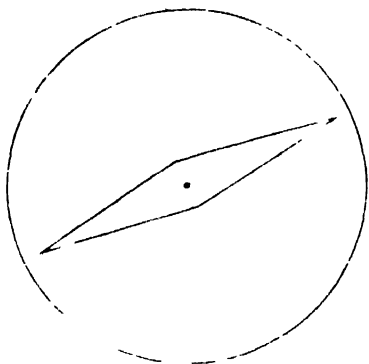


FIG. 5.

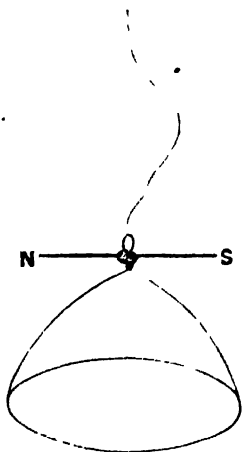
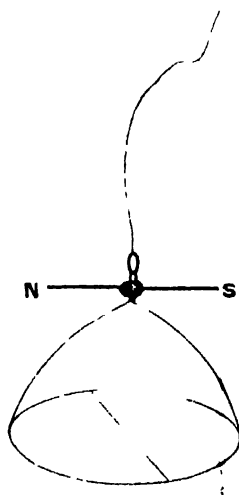


FIG. 6.



turn, it leaves the dust in the interior entirely behind. That shows that the water inside the ring offers no resistance to the shearing

action brought into play. The part of the surface of water immediately in contact with the ring no doubt goes round; but the movement spreads to a very little distance. The same would be observed if we added soap. But if I add some saponine, we shall find a different result, and that the behaviour of the dust in the interior of the ring is materially altered. The saponine has stiffened the surface, so that the ring turns with more difficulty; and when it turns, it carries round the whole interior with it. The surface has now got a stiffness from which it was free before; but the point upon which I wish to fix your attention is that the surface of pure water does not behave in the same way. If, however, we substitute for the simple hoop another provided with a material diameter (Fig. 6), lying also in the surface of the water, then we shall find, as was found by Plateau in his experiment, that the water is carried round. In this case, it is no longer possible for the surface to be left behind, as it was with the simple hoop, unless it is willing to undergo local expansions and contractions of area. The difference of behaviour proves that what a water surface resists is not shearing, but expansions and contractions; in fact, it behaves just as a contaminated surface should do. On this supposition, it is easy to explain the effects observed by Plateau; but the question at once arises, can we believe that all water surfaces hitherto experimented upon are sensibly contaminated? and if yes, is there any means by which the contamination may be removed? I cannot in the time at my disposal discuss this question fully, but I may say that I have succeeded in purifying the surface of the water in Plateau's experiment, until it behaved like alcohol. It is therefore certain that Plateau's superficial viscosity is due to contamination, as was conjectured by Marangoni.

I must now return to the subject of foam, from which I may seem to have digressed, though I have not really done so. Why does surface contamination enable a film to exist with greater permanence than it otherwise could? Imagine a vertical soap film. Could the film continue to exist if the tension were equal at all its parts? It is evident that the film could not exist for more than a moment; for the interior part, like the others, is acted on by gravity, and, if no other forces are acting, it will fall 16 feet in a second. If the tension above be the same as below, nothing can prevent the fall. But observation proves that the central parts do not fall, and thus that the tension is not uniform, but greater in the upper parts than in the lower. A film composed of pure liquid can have but a very brief life. But if it is contaminated, there is then a possibility of a different tension at the top and at the bottom, because the tension depends on the degree of contamination. Supposing that at the first moment the film were uniformly contaminated, then the central parts would begin to drop. The first effect would be to concentrate the contamination on the parts underneath and diminish it above. The result of that would be an increase of tension on the upper parts. So the effect would be to call a force into play tending to check the

motion, and it is only in virtue of such a force that a film can have durability. The main difference between a material that will foam and one that will not is in the liability of the surface to contamination from the interior.

I find my subject too long for my time, and must ask you to excuse the hasty explanations I have given at some parts. But I was anxious above all to show the principal experiments upon which are based the views that I have been led to entertain.

[RAYLEIGH.]

Friday, May 16, 1890.

EDWARD FRANKLAND, Esq. D.C.L. LL.D. F.R.S. Vice-President,  
in the Chair.

PROFESSOR RAPHAEL MILDOLA, F.R.S. M.R.I.

*The Photographic Image.*

THE history of a discovery which has been developed to such a remarkable degree of perfection as photography has naturally been a fruitful source of discussion among those who interest themselves in tracing the progress of science. It is only my presence in this lecture theatre, in which the first public discourse on photography was given by Thomas Wedgwood at the beginning of the century, that justifies my treading once again a path which has already been so thoroughly well beaten. If any further justification for trespassing upon the ground of the historian is needed, it will be found in the circumstance that in the autumn of last year there was held a celebration of what was generally regarded as the jubilee of the discovery. This celebration was considered by many to have reference to the public disclosure of the Daguerreotype process, made through the mouth of Arago to the French Academy of Sciences on August 10, 1839. There is no doubt that the introduction of this process marked a distinct epoch in the history of the art, and gave a great impetus to its subsequent development. But, while giving full recognition to the value of the discovery of Daguerre, we must not allow the work of his predecessors and contemporaries in the same field to sink into oblivion. After the lapse of half a century we are in a better position to consider fairly the influence of the work of different investigators upon modern photographic processes.

I have not the least desire on the present occasion to raise the ghosts of dead controversies. In fact, the history of the discovery of photography is one of those subjects which can be dealt with in various ways, according to the meaning assigned to the term. There is ample scope for the display of what Mr. Herbert Spencer calls the "bias of patriotism." If the word "photography" be interpreted literally as writing or inscribing by light, without any reference to the subsequent permanence of the inscription, then the person who first intentionally caused a design to be imprinted by light upon a photo-sensitive compound must be regarded as the first photographer. According to Dr. Eder, of Vienna, we must place this experiment to the credit of Johann Heinrich Schulze, the son of a German tailor, who was born in the Duchy of Magdeburg, in Prussia, in 1687, and who died in 1744, after a life of extraordinary activity as a linguist, theologian, physician, and philosopher. In the year 1727, when

experimenting on the subject of phosphorescence, Schulze observed that by pouring nitric acid, in which some silver had previously been dissolved, on to chalk, the undissolved earthy residue had acquired the property of darkening on exposure to light. This effect was shown to be due to light, and not to heat. By pasting words cut out in paper on the side of the bottle containing his precipitate, Schulze obtained copies of the letters on the silvered chalk. The German philosopher certainly produced what might be called a temporary photogram. Whatever value is attached to this observation in the development of modern photography, it must be conceded that a considerable advance was made by spreading the sensitive compound over a surface instead of using it in mass. It is hardly necessary to remind you here that such an advance was made by Wedgwood and Davy in 1802.\* The impressions produced by these last experimenters were, unfortunately, of no more permanence than those obtained by Schulze three-quarters of a century before them.

It will, perhaps, be safer for the historian of this art to restrict the term photograph to such impressions as are possessed of permanence: I do not, of course, mean absolute permanence, but ordinary durability in the common-sense acceptation of the term. From this point of view the first real photographs, i.e. permanent impressions of the camera picture, were obtained on bitumen films by Joseph Nicéphore Niepce, of Châlons-sur-Saône, who, after about twenty years' work at the subject, had perfected his discovery by 1826. Then came the days of silver salts again, when Daguerre, who commenced work in 1824, entered into a partnership with Niepce in 1829, which was brought to a termination by the death of the latter in 1833. The partnership was renewed between Daguerre and Niepce de St. Victor, nephew of the elder Niepce. The method of fixing the camera picture on a film of silver iodide on a silvered copper plate—the process justly associated with the name of Daguerre, was ripe for disclosure by 1838, and was actually made known in 1839.

The impartial historian of photography who examines critically into the evidence will find that, quite independently of the French pioneers, experiments on the use of silver salts had been going on in this country, and photographs, in the true sense, had been produced almost simultaneously with the announcement of the Daguerreotype process, by two Englishmen whose names are as household words in the ranks of science—I refer to William Henry Fox Talbot and Sir John Herschel. Fox Talbot commenced experimenting with silver salts on paper in 1834, and the following year he succeeded in imprinting the camera picture on paper coated with the chloride. In January 1839 some of his "photogenic drawings"—the first "silver prints" ever obtained—were exhibited in this Institution, by Michael

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\* "An Account of a Method of Copying Paintings upon Glass, and of making Profiles by the Agency of Light upon Nitrate of Silver. Invented by T. Wedgwood, Esq. With Observations by H. Davy." 'Journ. R. I.' 1802, p. 170.



Faraday. In the same month he communicated his first paper on a photographic process to the Royal Society, and in the following month he read a second paper before the same society, giving the method of preparing the sensitive paper and of fixing the prints. The outcome of this work was the "Calotype" or Talbotype process, which was sufficiently perfected for portraiture by 1840, and which was fully described in a paper communicated to the Royal Society in 1841. The following year Fox Talbot received the Rumford Medal for his "discoveries and improvements in photography." \*

Herschel's process consisted in coating a glass plate with silver chloride by subsidence. The details of the method, from Herschel's own notes, have been published by his son, Prof. Alexander Herschel.† By this means the old 40-foot telescope at Slough was photographed in 1839. By the kindness of Prof. Herschel, and with the sanction of the Science and Art Department, Herschel's original photographs have been sent here for your inspection. The process of coating a plate by allowing a precipitate to settle on it in a uniform film is, however, impracticable, and was not further developed by its illustrious discoverer. We must credit him, however, as being the first to use glass as a substratum. Herschel further discovered the important fact that while the chloride was very insensitive alone, its sensitiveness was greatly increased by washing it with a solution of silver nitrate. It is to Herschel, also, that we are indebted for the use of sodium thiosulphate as a fixing agent, as well as for many other discoveries in connection with photography, which are common matters of history.

Admitting the impracticability of the method of subsidence for producing a sensitive film, it is interesting to trace the subsequent development of the processes inaugurated about the year 1839. The first of photographic methods—the bitumen process of Niepce—survives at the present time, and is the basis of some of the most important of modern photo-mechanical printing processes. [Specimens illustrating photo-etching from Messrs. Waterlow and Sons exhibited.] The Daguerreotype process is now obsolete. As it left the hands of its inventor it was unsuited for portraiture, on account of the long exposure required. It is evident, moreover, that a picture on an opaque metallic plate is incapable of reproduction by printing through, so that in this respect the Talbotype possessed distinct advantages. This is one of the most important points in Fox Talbot's contributions to photography. He was the first to produce a transparent paper negative from which any number of positives could be obtained by printing through. The silver print of modern times is the lineal descendant of the Talbotype print. After forty

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\* For these and other details relating to Fox Talbot's work, necessarily excluded for want of time, I am indebted to his son Mr. C. H. Talbot, of Lacock Abbey.

† 'Photog. Journ. and Trans. Photog. Soc.,' June 15, 1872.

years' use of glass as a substratum, we are going back to Fox Talbot's plan, and using thin flexible films—not exactly of paper, but of an allied substance, celluloid. [Specimens of Talbotypes, lent by Mr. Crookes, exhibited, with celluloid negatives by the Eastman Company.]

If I interpret this fragment of history correctly, the founders of modern photography are the three men whose labours have been briefly sketched. The jubilee of last autumn marked a culminating point in the work of Niepce and Daguerre, and of Fox Talbot. The names of these three pioneers must go down to posterity as co-equal in the annals of scientific discovery. [Portraits by Mr. H. M. Elder shown.] The lecture theatre of the Royal Institution offers such tempting opportunities to the chronicler of the history of this wonderful art that I must close this treatment of the subject by reminding myself that in selecting the present topic I had in view a statement of the case of modern photography from its scientific side only. There is hardly any invention associated with the present century which has rendered more splendid services in every department of science. The physicist and chemist, the astronomer and geographer, the physiologist, pathologist, and anthropologist will all bear witness to the value of photography. The very first scientific application of Wedgwood's process was made here by the illustrious Thomas Young, when he impressed Newton's rings on paper moistened with silver nitrate, as described in his Bakerian Lecture to the Royal Society on November 21, 1803. Prof. Dewar has just placed in my hands the identical slide, with the Newton rings still visible, which he believes Young to have used in this classic experiment. [Shown.]

Our modern photographic processes depend upon chemical changes wrought by light on films of certain sensitive compounds. Bitumen, under this influence, becomes insoluble in hydrocarbon oils, as in the heliographic process of the elder Niepce. Gelatine mixed with potassium dichromate becomes insoluble in water on exposure to light, a property utilised in the photo-etching process introduced in 1852 by Fox Talbot, some of whose original etchings have been placed at my disposal by Mr. Crookes. [Shown.] Chromatized gelatine now plays a most important part in the autotype and many photo-mechanical processes. The salts of iron in the ferric condition undergo reduction to the ferrous state under the influence of light in contact with oxidizable organic compounds. The use of these iron salts is another of Sir John Herschel's contributions to photography (1842), the modern "blue print" and the beautiful platinotype being dependent on the photo-reducibility of these compounds. [Cyanotype print developed with ferri-cyanide.]

Of all the substances known to chemistry at the present time, the salts of silver are by far the most important in photography, on account of the extraordinary degree of sensitiveness to which they can be raised. The photographic image, with which it is my privilege to deal on this occasion, is that invisible impression produced by

the action of light on a film of a silver haloid. Many methods of producing such films have been in practical use since the foundation of the art in 1839. All these depend on the double decomposition between a soluble chloride, bromide, or iodide, and silver nitrate, resulting in the formation of the silver haloid in a vehicle of some kind, such as albumen (Niepce de St. Victor, 1848), or collodion on glass, as made practicable by Scott Archer in 1851. For twenty years this collodion process was in universal use; its history and details of manipulation, its development into a dry plate process by Colonel Russell in 1861, and into an emulsion process by Bolton and Sayce in 1864, are facts familiar to every one.

The photographic film of the present time is a gelatino-haloid (generally bromide) emulsion. If a solution of silver nitrate is added to a solution of potassium bromide and the mixture well shaken, the silver bromide coagulates, and rapidly subsides to the bottom of the liquid as a dense curdy precipitate. [Shown.] If instead of water we use a viscid medium, such as gelatine solution, the bromide does not settle down, but forms an emulsion, which becomes quite homogeneous on agitation. [Shown.] This operation, omitting all details of ripening, washing, &c., as well known to practical photographers, is the basis of all the recent photographic methods of obtaining negatives in the camera. The use of this invaluable vehicle, gelatine, was practically introduced by R. L. Maddox in 1871, previous experiments in the same direction having been made by Gaudin (1853-61). Such a gelatino-bromide emulsion can be spread uniformly over any substratum—glass, paper, gelatine, or celluloid—and when dry, gives a highly sensitive film.

The fundamental problem which fifty years' experience with silver haloid films has left in the hands of chemists is that of the nature of the chemical change which occurs when a ray of light falls on such a silver salt. Long before the days of photography—far back in the sixteenth century—Fabricius, the alchemist, noticed that native horn silver became coloured when brought from the mine and exposed. The fact presented itself to Robert Boyle in the seventeenth century, and to Beccarius, of Turin, in the eighteenth century. The change of colour undergone by the chloride was first shown to be associated with chemical decomposition in 1777, by Scheele, who proved that chlorine was given off when this salt darkened under water. I can show you this in a form which admits of its being seen by all. [Potassium iodide and starch paper were placed in a glass cell with silver chloride, and the arrangement exposed to the electric light till the paper had become blue.] The gas which is given off under these circumstances is either the free halogen or an oxide or acid of the halogen, according to the quantity of moisture present and the intensity of the light. I have found that the bromide affects the iodide and starch paper in the same way, but silver iodide does not give off any gas which colours the test paper. All the silver haloids become coloured on exposure to light, the change being most marked

in the chloride, less in the bromide, and least of all in the iodide. The latter must be associated with some halogen absorbent to render the change visible. [Strips of paper coated with the pure haloids, the lower halves brushed over with silver nitrate solution, were exposed.] The different degrees of coloration in the three cases must not be considered as a measure of the relative sensitiveness: it simply means that the products of photo-chemical change in the three haloids are inherently possessed of different depths of colour.

From the fact that halogen in some form is given off, it follows that we are concerned with photo-chemical decomposition, and not with a physical change only. All the evidence is in favour of this view. Halogen absorbents, such as silver nitrate on the lower halves of the papers in the last experiment, organic matter, such as the gelatine in an emulsion, and reducing agents generally, all accelerate the change of colour. Oxidizing and halogenizing agents, such as mercuric chloride, potassium dichromate, &c., all retard the colour change. [Silver chloride paper, painted with stripes of solutions of sodium sulphite, mercuric chloride, and potassium dichromate, was exposed.] It is impossible to account for the action of these chemical agents except on the view of chemical decomposition. The ray of light falling upon a silver haloid must be regarded as doing chemical work; the vibratory energy is partly spent in doing the work of chemical separation, and the light passes through a film of such haloid partly robbed of its power of doing similar work upon a second film. It is difficult to demonstrate this satisfactorily in the lecture-room, on account of the opacity of the silver haloids, but the work of Sir John Herschel, J. W. Draper, and others, has put it beyond doubt that there is a relationship of this kind between absorption and decomposition. It is well known, also, that the more refrangible rays are the most active in promoting the decomposition in the case of the silver haloids. This was first proved for the chloride by Scheele, and is now known to be true for the other haloids. It would be presumption on my part, in the presence of Captain Abney, to enlarge upon the effects of the different spectral colours on these haloids, as this is a subject upon which he can speak with the authority of an investigator. It only remains to add that the old idea of a special "actinic" force at the more refrangible end of the spectrum has long been abandoned. It is only because the silver haloids absorb these particular rays that the blue end of the spectrum is most active in promoting their decomposition. Many other instances of photo-chemical decomposition are known in which the less refrangible rays are the most active, and it is possible to modify the silver haloids themselves so as to make them sensitive for the red end of the spectrum.

The chemical nature of the coloured products of photo-chemical decomposition is still enshrouded in mystery. Beyond the fact that they contain less halogen than the normal salt, we are not much in advance of the knowledge bequeathed to us by Scheele in the last

century. The problem has been attacked by chemists again and again, but its solution presents extraordinary difficulties. These products are never formed—even under the most favourable conditions of division and with prolonged periods of exposure—in quantities beyond what the chemists would call “a mere trace.” Their existence appears to be determined by the great excess of unaltered haloid with which they are combined. Were I to give free rein to the imagination, I might set up the hypothesis that the element silver is really a compound body invariably containing a minute percentage of some other element, which resembles the compound which we now call silver in all its chemical reactions, but alone is sensitive to light. I offer this suggestion for the consideration of the speculative chemist.\* For the coloured product as a whole, i. e. the product of photo-decomposition with its combined unchanged haloid, Carey Lea has proposed the convenient term “photosalt.” It will avoid circumlocution if we adopt this name. The photosalts have been thought at various times to contain metallic silver, allotropic silver, a sub-haloid, such as argentous chloride, &c., or an oxyhaloid. The free metal theory is disposed of by the fact that silver chloride darkens under nitric acid of sufficient strength to dissolve the metal freely. The acid certainly retards the formation of the photosalt, but does not prevent it altogether. When once formed the photo-chloride is but slowly attacked by boiling dilute nitric acid, and from the dry photosalt mercury extracts no silver. The assumption of the existence of an allotropic form of silver insoluble in nitric acid cannot be seriously maintained. The sub-haloid theory of the product may be true, but it has not yet been established with that precision which the chemist has a right to demand. We must have analyses giving not only the percentage of halogen, but also the percentage of silver, in order that it may be ascertained whether the photosalt contains anything besides metal and halogen. The same may be said of the oxyhaloid theory: it may be true, but it has not been demonstrated.

The oxyhaloid theory was first suggested by Robert Hunt† for the chloride; it was taken up by Sahler, and has recently been revived by Dr. W. R. Hodgkinson. It has been thought that this theory is disposed of by the fact that the chloride darkens under liquids, such as hydrocarbons, which are free from oxygen. I have been repeating some of these experiments with various liquids, using every possible precaution to exclude oxygen and moisture; dry silver chloride heated to incipient fusion has been sealed up in tubes in dry benzene,

\* I have gone so far as to test this idea experimentally in a preliminary way, the result being, as might have been anticipated, negative. Silver chloride, well darkened by long exposure, was extracted with a hot saturated solution of potassium chloride, and the dissolved portion, after precipitation by water, compared with the ordinary chloride by exposure to light. Not the slightest difference was observable either in the rate of coloration or in the colours of the products. Perhaps it may be thought worth while to repeat the experiment, using a method analogous to the “method of fractionation” of Crookes.

† ‘Researches on Light,’ 2nd ed. 1854, p. 80.

petroleum, and carbon tetrachloride and exposed since March. [Tubes shown.] In all cases the chloride has darkened. The salt darkens, moreover, in a Crookesian vacuum.\* By these experiments the oxychloride theory may be scotched, but it is not yet killed; the question now presents itself, whether the composition of the photosalt may not vary according to the medium in which it is generated. Analogy sanctions the supposition that when the haloid darkens under water or other oxygen-containing liquid, or even in contact with moist or dry air, that an oxychloride may be formed, and enter into the composition of the photosalt. The analogy is supplied by the corresponding salt of copper, viz. cuprous chloride, which darkens rapidly on exposure. [Design printed on flat cell filled with cuprous chloride by exposure to electric light.] Wohler conjectured that the darkened product was an oxychloride, and this view receives a certain amount of indirect support from these tubes [shown], in which dry cuprous chloride has been sealed up in benzene and carbon tetrachloride since March; and although exposed in a southern window during the whole of that time, the salt is as white as when first prepared. Some cuprous chloride sealed up in water, and exposed for the same time, is now almost black. [Shown.]

When silver is precipitated by reduction in a finely divided state in the presence of the haloid, and the product treated with acids, the excess of silver is removed and coloured products are left which are somewhat analogous to the photosalts proper. These coloured haloids are also termed by Carey Lea photosalts because they present many analogies with the coloured products of photo-chemical change. Whether they are identical in composition it is not yet possible to decide, as we have no complete analyses. The first observations in this direction were published more than thirty years ago in a report by a British Association Committee,† in which the red and chocolate-coloured chlorides are distinctly described. Carey Lea has since contributed largely to our knowledge of these coloured haloids, and has at least made it appear highly probable that they are related to

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\* Some dry silver chloride which Mr. Crookes has been good enough to send up for me in a high vacuum, darkens on exposure quite as rapidly as the dry salt in air. It soon regains its original colour when kept in the dark. It behaves, in fact, just as the chloride is known to behave when sealed up in chlorine, although its colour is of course much more intense after exposure than is the case with the chloride in chlorine. The tube in which the chloride had been sealed up in benzene, gave off a considerable quantity of hydrogen chloride on breaking the point in June.

† These results were arrived at in three ways. In one case hydrogen was passed through silver citrate suspended in hot water, and the product extracted with citric acid. "The result of treating the residue with chlorhydric acid, and then dissolving the silver by dilute nitric acid, was a rose-tinted chloride of silver." In another experiment the dry citrate was heated in a stream of hydrogen at 212° F., and the product, which was partly soluble in water, gave a brown residue, which furnished "a very pale red body on being transformed by chlorhydric and nitric acids." In another experiment silver arsenite was formed, this being treated with caustic soda, and the black precipitate then treated successively with

the products formed by the action of light. [Red photo-chloride and purple photobromide and iodide shown.]

The photographic image is impressed on a modern film in an inappreciable fraction of a second, whereas the photosalt requires an appreciable time for its production. The image is invisible simply because of the extremely minute quantity of haloid decomposed. In the present state of knowledge it cannot be asserted that the material composing this image is identical in composition with the photosalt, for we know the composition of neither the one nor the other. But they are analogous in so far as they are both the result of photo-chemical decomposition, and there is great probability that they are closely related, if not identical, chemically. It may turn out that there are various kinds of invisible images, according to the vehicle or halogen absorbent—in other words, according to the sensitiser with which the silver haloid is associated. The invisible image is revealed by the action of the developer, into the function of which I do not propose to enter. It will suffice to say that the final result of the developing solution is to magnify the deposit of photosalt by accumulating metallic silver thereon by accretion or reduction. Owing to the circumstance that the image is impressed with such remarkable rapidity, and that it is invisible when formed, it has been maintained, and is still held by many, that the first action of light on the film is molecular or physical, and not chemical. The arguments in favour of the chemical theory appear to me to be tolerably conclusive, and I will venture to submit a few of them.

The action of reagents upon the photographic film is quite similar to the action of the same reagents upon the silver haloids when exposed to the point of visible coloration. Reducing agents and halogen absorbents increase the sensitiveness of the film: oxidising and halogenising agents destroy its sensitiveness. It is difficult to see on the physical theory why it should not be possible to impress an image on a film, say of pure silver bromide, as readily as on a film of the same haloid embedded in gelatine. Every one knows that this cannot be done. I have myself been surprised at the extreme insensitiveness of films of pure bromide prepared by exposing films of silver deposited on glass to the action of bromine vapour. On the chemical theory we know that gelatine is a splendid sensitiser—i. e. bromine absorbent. There is another proof which has been in our hands for nearly thirty years, but I do not think it has been viewed in this light before. It has been shown by Carey Lea, Eder, and especially by Abney—who has investigated the matter most

chlorhydric and nitric acids: "Silver is dissolved, and there is left a substance . . . [of] a rich chocolate or maroon, &c." This on analysis was found to contain 24 per cent. of chlorine, the normal chloride requiring 24.74 and the subchloride 14.08 per cent. The committee which conducted these experiments consisted of Messrs. Maskelyne, Hadow, Hardwich, and Llewelyn. 'B. A. Rep.' 1859, p. 103.

thoroughly—that a shearing stress applied mechanically to a sensitive film leaves an impression which can be developed in just the same way as though it had been produced by the action of light. [Pressure marks on Eastman bromide paper developed by ferrous oxalate.] Now that result cannot be produced on a surface of the pure haloid; some halogen absorbent, such as gelatine, must be associated with the haloid. We are concerned here with a chemical change of that class so ably investigated by Prof. Spring, of Liège, who has shown that by mere mechanical pressure it is possible to bring about chemical reaction between mixtures of finely divided solids.\* Then again, mild reducing agents, too feeble to reduce the silver haloids directly to the metallic state, such as alkaline hypophosphites, glucose or lactose, and alkali, &c., form invisible images which can be developed in precisely the same way as the photographic image. All this looks like chemical change, and not physical modification pure and simple.

I have in this discourse stoically resisted the tempting opportunities for pictorial display which the subject affords. My aim has been to summarise the position in which we find ourselves with respect to the invisible image after fifty years' practice of the art. This image is, I venture to think, the property of the chemist, and by him must the scientific foundation of photography be laid. We may not be able to give the formula of the photosalt, but if the solution of the problem has hitherto eluded our grasp it is because of the intrinsic difficulties of the investigation. The photographic image brings us face to face—not with an ordinary, but with an extraordinary class of chemical changes due entirely to the peculiar character of the silver salts. The material composing the image is not of that definite nature with which modern chemical methods are in the habit of dealing. The stability of the photosalt is determined by some kind of combination between the sub-haloid or oxy-haloid, or whatever it may be, and the excess of unaltered haloid which enters into its composition. The formation of the coloured product presents certain analogies with the formation of a saturated solution; the product of photo-chemical decomposition is formed under the influence of light up to a certain percentage of the whole photosalt, beyond which it cannot be increased—in other words the silver haloid is saturated by a very minute percentage of its own product of photo-decomposition. The photosalt belongs to a domain of chemistry—a no-man's land—peopled by so-called “molecular compounds,” into which the pure chemist ventures but timidly. But these compounds are more and more urging their claims for consideration, and sooner or later they will have to be reckoned with, even if

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\* The connection between the two phenomena was suggested during a course of lectures delivered by me two years ago (*Chemistry of Photography*, p. 191). I have since learnt that the same conclusion had been arrived at independently, by Mr. C. H. Bothamley, of the Yorkshire College, Leeds.



they lack that definiteness which the modern chemist regards as the essential criterion of chemical individuality. The investigation may lead to the recognition of a new order of chemical attraction, or of the old chemical attraction in a different degree. The chemist who discourses here upon this subject at the end of the half-century of photography into which we have now entered, will no doubt know more about this aspect of chemical affinity; and if I may invoke the spirit of prophecy in concluding, I should say that a study of the photographic film with its invisible image will have contributed materially to its advancement.

[R. M.]

Friday, January 30, 1891.

EDWARD FRANKLAND, Esq. D.C.L. LL.D. F.R.S. Vice-President,  
in the Chair.

PROFESSOR JOHN W. JUDD, F.R.S. F.G.S.

*The Rejuvenescence of Crystals.*

VERY soon after the invention of the microscope, the value of that instrument in investigating the phenomena of crystallisation began to be recognised. The study of crystal-morphology and crystallogenesi was initiated in this country by the observations of Robert Boyle; and since his day, a host of investigators—among whom may be especially mentioned Leeuenhoek and Vogelsang in Holland, Link and Frankenheim in Germany, and Pasteur and Senarmont in France—have added largely to our knowledge of the origin and development of crystalline structures. Nor can it be said with justice that this field of investigation, opened up as it was by English pioneers, has been ignobly abandoned to others; for the credit of British science has been fully maintained by the numerous and brilliant discoveries in this department of knowledge of Brewster and Sorby.

There is no branch of science which is more dependent for its progress on a knowledge of the phenomena of crystallisation than geology. In seeking to explain the complicated phenomena exhibited by the crystalline masses composing the earth's crust, the geologist is constantly compelled to appeal to the physicist and chemist—from them alone can he hope to obtain the light of experiment and the leading of analogy whereby he may hope to solve the problems which confront him.

But if geology owes much to the researches of those physicists and chemists who have devoted their studies to the phenomena of crystallisation, the debt has been more than repaid through the new light which has been thrown on these questions by the investigation of naturally-formed crystals by mineralogists and geologists.

In no class of physical operations is *time* such an important factor as in crystallisation; and Nature, in producing her inimitable examples of crystalline bodies, has been unsparing in her expenditure of time. Hence it is not surprising to find that some of the most wonderful phenomena of crystallisation can best be studied—some indeed can only be studied—in those exquisite specimens of Nature's handiwork which have been slowly elaborated by her during periods which must be measured in millions of years.

I propose to-night to direct your attention to a very curious case in which a strikingly complicated group of phenomena is presented in

a crystalline mass; and these phenomena, which have been revealed to the student of natural crystals, are of such a kind that we can scarcely hope to reproduce them in our test-tubes and crucibles.

But if we cannot expect to imitate all the effects which have in this case been slowly wrought out in Nature's laboratory, we can at least investigate and analyse them; and, in this way, it may be possible to show that phenomena like those in question must result from the possession by crystals of certain definite properties. Each of these properties, we shall see, may be severally illustrated and experimentally investigated, not only in natural products, but in the artificially-formed crystals of our laboratories.

In order to lead up to the explanation of the curious phenomena exhibited by the rock mass in question, the first property of crystals to which I have to refer may be enunciated as follows:—

*Crystals possess the power of resuming their growth after interruption; and there appears to be no limit to the time after which this resumption of growth may take place.*

It is a familiar observation that if a crystal be taken from a solution and put aside, it will, if restored after a longer or shorter interval to the same or a similar solution, continue to increase as before. But geology affords innumerable instances in which this renewal of growth in crystals has taken place after millions of years must have elapsed. Still more curious is the fact, of which abundant proof can be given, that a crystal formed by one method may, after a prolonged interval, continue its growth under totally different conditions and by a very different method. Thus crystals of quartz, which have clearly been formed in a molten magma, and contain enclosures of glass, may continue their growth when brought in contact with solutions of silica at ordinary temperatures. In the same way, crystals of felspar which have been formed in a mass of incandescent lava, may increase in size when solvent agents bring to them the necessary materials from an enveloping mass of glass, even after the whole mass has become cold and solid.

It is this power of resuming growth after interruption, which leads to the formation of zoned crystals, like the fine specimen of amethyst enclosed in colourless quartz, which was presented to the Royal Institution seventy years ago by Mr. Snodgrass.

The growth of crystals, like that of plants and animals, is determined by their environment; the chief conditions affecting their development being temperature, rate of growth, the supply of materials (which may vary in quality as well as quantity), and the presence of certain foreign bodies.

It is a very curious circumstance that the form assumed by a crystal may be completely altered by the presence of infinitesimal traces of certain foreign substances—foreign substances, be it remarked, which do not enter in any way into the composition of the crystallising mass. Thus there are certain crystals which can only be formed in the presence of water, fluorides, or other salts. Such foreign bodies,

which exercise an influence on a crystallising substance without entering into its composition, have been called by the French geologists "mineralisers." Their action seems to curiously resemble that of diastase, and of the bodies known to chemists as "ferments," so many of which are now proved to be of organic origin.

Studied according to their mode of formation, zoned crystals fall naturally into several different classes.

In the first place, we have the cases in which the successive shells or zones differ only in colour or some other accidental character. Sometimes such differently coloured shells of the crystal are sharply cut off from one another; while, in other instances, they graduate imperceptibly one into the other.

A second class of zoned crystals includes those in which we find clear evidence that there have been pauses, or at all events changes in the rate of their growth. The interruption in growth may be indicated in several different ways. One of the commonest of these is the formation of cavities filled with gaseous, liquid, or vitreous material,—according to the way the crystal has been formed, by volatilisation, by solution, or by fusion; the production of these cavities indicating rapid or irregular growth. Not unfrequently, it is clear that the crystal, after growing to a certain size, has been corroded or partially resorbed in the mass in which it is being formed, before its increase was resumed. In other cases, a pause in the growth of the crystal is indicated by the formation of minute foreign crystals, or the deposition of uncrystallised material along certain zonal planes in the crystal.

Some very interesting varieties of minerals—like the Cotterite of Ireland, the red quartz of Cumberland, and the spotted amethyst of Lake Superior—can be shown to owe their peculiarities to thin bands of foreign matter zonally included in them during their growth.

A curious class of zoned crystals arises when there is a change in the *habit* of a crystal during its growth. Thus, as Lavallo showed in 1851,\* if an octahedron of alum be allowed to grow to a certain size in a solution of that substance, and then a quantity of alkaline carbonate be added to the liquid, the octahedral crystal, without change in the length of its axes, will be gradually transformed into a cube. In the same way, a scalenohedron of calcite may be found enclosed in a prismatic crystal of the same mineral, the length of the vertical axis being the same in both crystals.

By far the most numerous and important class of zoned crystals is that which includes the forms where the successive zones are of different, though analogous, chemical composition. In the case of the alums and garnets, we may have various *isomorphous* compounds forming the successive zones in the same crystal; while in substances crystallising in other systems than the cubic, we find *pleiomorphous* compounds forming the different enclosing shells. Such cases are

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\* Bull. Géol. Soc. Paris, 2nd ser. vol. viii. pp. 610-13.

illustrated by many artificial crystals, and by the tourmalines, the epidotes, and the feldspars among minerals. The separate zones, consisting of different materials, are sometimes separated by well-marked planes, but in other cases they shade imperceptibly into one another.

In connection with this subject, it may be well to point out that zoned crystals may be formed of two substances which do not crystallise in the same system. Thus crystals of the monoclinic angite may be found surrounded by a zone of the rhombic enstatite; and crystals of a triclinic feldspar may be found enlarged by an outer shell of monoclinic feldspar.

Still more curious is the fact that, where there is similarity in crystalline form, and an approximation in the dominant angles (plesiomorphism), we may have zoning and intergrowth in the crystals of substances which possess no chemical analogy whatever. Thus, as Senarmont showed in 1856, a cleavage-rhomb of the natural calcic carbonate (calcite), when placed in a solution of the sodic nitrate, becomes enveloped in a zone of this latter substance; and Tschermak has proved that the compound crystal thus formed behaves like a homogeneous one, if tested by its cleavage, by its susceptibility to twin lamellation, or by the figures produced by etching. In the same way, zircons, which are composed of the two oxides of silicon and zirconium, are found grown in composite crystals with xenotime, a phosphate of the metals of the cerium and yttrium groups.

These, and many other similar facts which might be adduced, point to the conclusion that the beautiful theory of isomorphism, as originally propounded by Mitscherlich, stands in need of much revision as to many important details; it may be indeed of complete reconstruction in the light of modern observation and experiment.

The second property of crystals to which I must direct your attention is the following:—

*If a crystal be broken or mutilated in any way whatever, it possesses the power of repairing its injuries during subsequent growth.*

As long ago as 1836, Frankenhofm showed that if a drop of a saturated solution be allowed to evaporate on the stage of a microscope, the following interesting observations may be made upon the growing crystals. When they are broken up by a rod, each fragment tends to re-form as a perfect crystal; and if the crystals be caused to be partially redissolved by the addition of a minute drop of the mother-liquor, further evaporation causes them to resume their original development.\*

In 1842 Hermann Jordan showed that crystals taken from a solution and mutilated, gradually became repaired or healed when replaced in the solution.† Jordan's observations, which were

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\* Pogg. Ann., Bd. xxxvii. (1836).

† Muller, Archiv for 1842, pp. 46-56.

published in a medical journal, do not however seem to have attracted much attention from the physicists and chemists of the day.

Lavalle, between the years 1850 and 1853,\* and Kopp in the year 1855,† made a number of valuable observations bearing on this interesting property of crystals. In 1856 the subject was more thoroughly studied by three investigators, who published their results almost simultaneously—these were Marbach,‡ Pasteur,§ and Senarmont.|| They showed that crystals taken from a solution and mutilated in various ways, upon being restored to the liquid, became completely repaired during subsequent growth.

As long ago as 1851, Lavalle had asserted that when one solid angle of an octahedron of alum is removed, the crystal tends to reproduce the same mutilation on the opposite angle when its growth is resumed. The phenomena observed have, by some subsequent writers, however, been explained in another way to that suggested by the author of this experiment. In the same way the curious experiments performed at a subsequent date by Karl von Hauer—experiments which led him to conclude that hemihedrism and other peculiarities in crystal growth might be induced by mutilation,¶—have been asserted by other physicists and chemists not to justify the startling conclusions drawn from them at the time. It must be admitted that new experiments bearing on this interesting question are, at the present time, greatly needed.

In 1881 Loir demonstrated two very important facts with regard to growing crystals of alum.\*\* *First*, that if the injuries in such a crystal be not too deep, it does not resume growth over its general surface until those injuries have been repaired. *Secondly*, that the injured surfaces of crystals grow more rapidly than natural faces. This was proved by placing artificially cut octahedra and natural crystals of the same size in a solution, and comparing their weight after a certain time had elapsed.

The important results of this capacity of crystals for undergoing healing and enlargement, and their application to the explanation of interesting geological phenomena, has been pointed out by many authors. Sorby has shown that, in the so-called crystalline sand-grains, we

\* Bull. Géol. Soc. Paris, 2nd ser., vol. viii. (1851), pp. 610-13; Moigno, *Cosmos*, ii. (1853) pp. 454-6; *Compt. Rend.*, xxxvi. (1853), pp. 493-5.

† Liebig. *Ann.*, xciv. (1855), pp. 118-25.

‡ *Compt. Rend.*, xliii. (1856), pp. 705-6, 800-2.

§ *Ibid.*, pp. 795-800.

|| *Ibid.*, p. 799.

¶ *Wien. Sitzungsber.*, xxxix. (1860), pp. 611-22; Erdmann, *Journ. Prakt. Chem.*, lxxxi., pp. 356-62; *Wien. Geol. Verhandl.*, xii. pp. 212-3, &c.; Compare Frankenheim, *Pogg. Ann.*, cxiii. (1861); Fr. Scharff, *Pogg. Ann.*, cx. (1860), pp. 529-38; *Neues Jahrb. für Min., &c.*, 1876, p. 24; and W. Sauber, *Liebig. Ann.*, cxxiv. (1862), pp. 78-82; also W. Ostwald, *Lehrbuch d. Allg. Chem.* (1885), Bd. i., p. 738; and O. Lehmann, *Molekular Physik* (1888), Bd. i. p. 312.

\*\* *Compt. Rend.*, Bd. xcii. p. 1166.

have broken and worn crystals of quartz, which, after many vicissitudes and the lapse of millions of years, have grown again, and been enveloped in a newly formed quartz-crystal. Bonney has shown how the same phenomena are exhibited in the case of mica, Becke and Whitman Cross in the case of hornblende, and Merrill in the case of angite. In the feldspars of certain rocks, it has been proved that crystals that have been rounded, cracked, corroded, and internally altered—which have, in short, suffered both mechanical and chemical injuries—may be repaired and enlarged with material that differs considerably in chemical composition from the original crystal.

It is impossible to avoid a comparison between these phenomena of the inorganic world and those so familiar to the biologist. It is only in the lowest forms of animal life that we find an unlimited power of repairing injuries; in the rhizopods and some other groups, a small fragment may grow into a perfect organism. In plants the same phenomenon is exhibited much more commonly, and in forms belonging to groups high up in the vegetable series. Thus parts of a plant, such as buds, bulbs, slips, and grafts, may—sometimes after a long interval—be made to grow up into new and perfect individuals. But in the mineral kingdom we find the same principle carried to a much farther extent. We know in fact no limit to the minuteness of fragments which may, under favourable conditions, grow into perfect crystals; no bounds as to the time during which the crystalline growth may be suspended in the case of any particular individual.

The next property of crystals which I must illustrate, in order to explain the particular case to which I am calling your attention to-night, is the following:—

*Two crystals of totally different substances may be developed within the space bounded by certain planes, becoming almost inextricably intergrown, though each retains its distinct individuality.*

This property is a consequence of the fact that the substance of a crystal is not necessarily continuous within the space enclosed by its bounding planes. Crystals often exhibit cavities filled with air and other foreign substances. In the calcite crystal found in the Fontainebleau sandstone, less than 40 per cent. of their mass consists of calcic carbonate, while more than 60 per cent. is made up of grains of quartz-sand caught up during crystallisation. In the rock called "graphic granite" we have the minerals orthoclase and quartz intergrown in such a way that the more or less isolated parts of each can be shown, by their optical characters, to be parts of great mutually interpenetrant crystals. Similar relations are shown in the so-called micrographic or micropegmatitic intergrowth of the same minerals which are so beautifully exhibited in the rock under our consideration this evening.

There is still another property of crystals that must be kept in

mind if we would explain the phenomena exhibited by this interesting rock.

*A crystal may undergo the most profound internal changes, and these may lead to great modifications of the optical and other physical properties of the mineral: yet so long as a small—often a very small—proportion of its molecules remain intact, the crystal may retain, not only its outward form, but its capacity for growing and repairing injuries.*

Crystals, like ourselves, grow old. Not only do they suffer from external injuries, mechanical fractures, and chemical corrosion, but from actions which affect the whole of their internal structure. Under the action of the great pressures in the earth's crust the minerals of deep-seated rocks are completely permeated by fluids which chemically react upon them. In this way negative crystals are formed in their substance (similar to the beautiful "ice-flowers" which are formed when a block of ice is traversed by a beam from the sun or an electric lamp), and these become filled with secondary products. As the result of this action, crystals, once perfectly clear and translucent, have acquired cloudy, opalescent, iridescent, aventurine, and "schiller" characters, and minerals thus modified abound in the rocks that have at any period of their history been deep-seated. As the destruction of their internal structure goes on, the crystals gradually lose more and more of their distinctive optical and their physical properties, retaining, however, their external form; till at last—when the last of the original molecules has been transformed or replaced by others—they pass into those mineral corpses known to us as "pseudomorphs."

But while crystals resemble ourselves in "growing old," and at last undergoing dissolution, they exhibit the remarkable power of growing young again, which we, alas! never do. This is in consequence of the following remarkable attribute of crystalline structures:—

*It does not matter how far internal change and disintegration may have gone on in a crystal; if only a certain small proportion of the unaltered molecules remain, the crystal may renew its youth and resume its growth.*

When old and much-altered crystals begin to grow again, the newly-formed material exhibits none of those marks of "senility" to which I have referred. The sand-grains that have been battered and worn into microscopic pebbles, and have been rendered cloudy by the development of millions of secondary fluid-cavities, may have clear and fresh quartz deposited upon them to form crystals, with exquisitely perfect faces and angles. The white, clouded, and altered felspar-crystals may, in the same way, be enveloped by a zone of clear and transparent material, which has been added millions of years after the first formation and the subsequent alteration of the original crystals. In these, and many similar examples which might be cited, the kernel, representing the original crystal, may exhibit evidence of having undergone the most profound chemical and physical modification, while the outer shell displays all the normal characteristics of the mineral.



We are now in a position to explain the particular case which I have thought of sufficient importance to claim your attention to-night.

In the Island of Mull, in the Inner Hebrides, there exist masses of granite of Tertiary age which are of very great interest to the geologist and mineralogist. In many places this granite exhibits beautiful illustrations of the curious intergrowths of quartz and felspar of which I have already spoken. Such parts of the rock often abound with cavities (druses), which I believe are not of original, but of secondary origin. At all events, it can be shown that these cavities have been localities in which crystal growth has gone on—they constitute indeed veritable laboratories of synthetic mineralogy.

Now in such cavities, the interpenetrant crystals of quartz and felspar in the rock have found a space where they may grow and complete their outward form; and it is curious to see how sometimes the quartz has prevailed over the felspar, and a pure quartz-crystal has been produced; while at other times the opposite effect has resulted, and a pure felspar individual has grown up. In these last cases, however much the original felspar may have been altered (kaolinised and rendered opaque), it is found to be completed by a zone of absolutely clear and unaltered felspar-substance. The result is that the cavities of the granite are lined with a series of projecting crystals of quartz and clear felspar, the relations of which to the similar materials in an altered condition composing the substance of the solid rock are worthy of the most careful observation and reflection. These relations can be fully made out when thin sections of the rock are examined under the microscope by the aid of polarised light, and they speak eloquently of the possession by the crystals in question of all those curious peculiarities of which I have reminded you this evening.

By problems such as those which we have endeavoured to solve to-night, the geologist is beset at every step. The crust of our globe is built up of crystals and crystal-fragments—of crystals in every stage of development, of growth, and of variation—of crystals, undergoing change, decay, and dissolution. Hence the study of the natural history of crystals must always constitute one of the main foundations of geological science; and the future progress of that science must depend on how far the experiments carried on in laboratories can be made to illustrate and explain our observations in the field.

[J. W. J.]

Friday, February 6, 1891.

SIR FREDERICK BRAMWELL, Bart. D.C.L. F.R.S. Honorary Secretary  
and Vice-President, in the Chair.

THE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. F.R.S. M.R.I.

PROFESSOR OF NATURAL PHILOSOPHY &c.

*Some Applications of Photography.*

ONE of the subjects to which I propose to invite your attention this evening is the application of instantaneous photography to the illustration of certain mechanical phenomena which pass so quickly as to elude ordinary means of observation. The expression "instantaneous photography" is perhaps not quite a defensible one, because no photography can be really instantaneous—some time must always be occupied. One of the simplest and most commonly used methods of obtaining very short exposures is by the use of movable shutters, for which purpose many ingenious mechanical devices have been invented. About two years ago we had a lecture from Prof. Muirbridge, in which he showed us the application of this method—and a remarkably interesting application it was—to the examination of the various positions assumed by a horse in his several gaits. Other means, however, may be employed to the same end, and one of them depends upon the production of an instantaneous light. It will obviously come to the same thing whether the light to which we expose the plates be instantaneous, or whether by a mechanical device we allow the plate to be submitted to a continuous light for only a very short time. A good deal of use has been made in this way of what is known as the magnesium flash light. A cloud of magnesium powder is ignited, and blazes up quickly with a bright light of very short duration. Now I want to compare that mode of illumination with another, in order to be able to judge of the relative degree of instantaneity, if I may use such an expression. We will illumine for a short time a revolving disc, composed of black and white sectors; and the result will depend upon how quick the motion is as compared with the duration of the light. If the light could be truly instantaneous, it would of necessity show the disc apparently stationary. I believe that the duration of this light is variously estimated at from one-tenth to one-fiftieth of a second; and as the arrangement that I have here is one of the slowest, we may assume that the time occupied will be about a tenth of a second. I will say the words one, two, three, and at the word three Mr. Gordon will project the

powder into the flame of a spirit lamp, and the flash will be produced. Please give your attention to the disc, for the question is whether the present uniform grey will be displaced by a perception of the individual black and white sectors. [Experiment.] You see the flash was *not* instantaneous enough to resolve the grey into its components.

I want now to contrast with that mode of illumination one obtained by means of an electric spark. We have here an arrangement by which we can charge Leyden jars from a Wimshurst machine. When the charge is sufficient, a spark will pass inside a lantern, and the light proceeding from it will be condensed and thrown upon the same revolving disc as before. The test will be very much more severe; but severe as it is, I think we shall find that the electric flash will bear it. The teeth on the outside of the disc are very numerous, and we will make them revolve as fast as we can, but we shall find that under the electric light they will appear to be absolutely stationary. [Experiment.] You will agree that the outlines of the black and white sectors are seen perfectly sharp.

Now, by means of this arrangement we might investigate a limit to the duration of the spark, because with a little care we could determine how fast the teeth are travelling—what space they pass through in a second of time. For this purpose it would not be safe to calculate from the multiplying gear on the assumption of no slip. A better way would be to direct a current of air upon the teeth themselves, and make them give rise to a musical note, as in the so-called siren. From the appearance of the disc under the spark we might safely say, I think, that the duration of the light is less than a tenth of the time occupied by a single tooth in passing. But the spark is in reality much more instantaneous than can be proved by the means at present at our command. In order to determine its duration, it would be necessary to have recourse to that powerful weapon the revolving mirror; and I do not, therefore, propose to go further into the matter to-night.

Experiments of this kind were made some twenty years ago by Prof. Rood, of New York, both on the duration of the discharge of a Leyden jar, and also on that of lightning. Prof. Rood found that the result depended somewhat upon the circumstances of the case; the discharge of a small jar being generally more instantaneous than that of a larger one. He proved that in certain cases the duration of the principal part of the light was as low as one twenty-five-millionth part of a second of time. That is a statement which probably conveys very little of its real meaning. A million seconds is about twelve days and nights. Twenty-five million seconds is nearly a year. So that the time occupied by the spark in Prof. Rood's experiment is about the same fraction of one second that one second is of a year. In many other cases the duration was somewhat greater; but in all his experiments it was well under the one-millionth part of a second. In certain cases you may have multiple sparks. I do not refer to the oscillating discharges of which Prof. Lodge gave us so

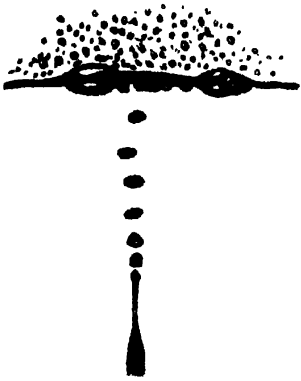


Fig 1

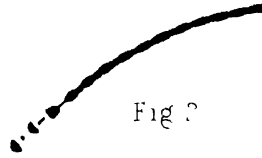


Fig 2

Fig 3



Fig 4



interesting an account last year; Prof. Rood's multiple discharge was not of that character. It consisted of several detached overflows of his Leyden jar when charged by the Rhumkorff coil. One number mentioned for the total duration was one six-thousandth part of a second; but the individual discharges had the degree of instantaneity of which I have spoken.

It is not a difficult matter to adapt the electrical spark to instantaneous photography. We will put the lantern into its proper position, excite the electric sparks within it, causing them to be condensed by the condenser of the lantern on to the photographic lens. We will then put the object in front of the lantern-condenser, remove the cap from the lens, expose the plate to the spark when it comes, and thus obtain an instantaneous view of whatever may be going on. I propose to go through the operation of taking such a photograph presently. I will not attempt any of the more difficult things of which I shall speak, but will take a comparatively easy subject,—a stream of bubbles of gas passing up through a liquid. In order that you may see what this looks like when observed in the ordinary way, we have arranged it here for projection upon the screen. [Experiment.] The gas issues from the nozzle, and comes up in a stream, but so fast that you cannot fairly see the bubbles. If, however, we take an instantaneous picture, we shall find that the stream is decomposed into its constituent parts. We arrange the trough of liquid in front of the lantern which contains the spark-making apparatus —[Experiment]—and we will expose a plate, though I hardly expect a good result in a lecture. A photographer's lamp provides some yellow light to enable us to see when other light is excluded. There goes the spark; the plate is exposed, and the thing is done. We will develop the plate, and see what it is good for; and if it turns out fit to show, we will have it on the screen within the hour.

In the meantime, we will project on the screen some slides taken in the same way and with the same subject. [Photograph shown.] That is an instantaneous photograph of a stream of bubbles. You see that the bubbles form at the nozzle from the very first moment, contrasting in that respect with the behaviour of jets of water, projected into air. [Fig. 1, Plate I.]

The latter is our next subject. This is the reservoir from which the water is supplied. It issues from a nozzle of drawn-out glass, and at the moment of issue it consists of a cylindrical body of water. The cylindrical form is unstable, however, and the water rapidly breaks up into drops, which succeed one another so rapidly that they can hardly be detected by ordinary vision. But by means of instantaneous photography the individual drops can be made evident. I will first project the jet itself on the screen, in order that you may appreciate the subject which we shall see presently represented by photography. [Experiment.] Along the first part of its length the jet of water is continuous. After a certain point it breaks into drops, but you cannot see them because of their rapidity. If we act on the jet with

a vibrating body, such as a tuning fork, the breaking into drops occurs still earlier, the drops are more regular, and assume a curious periodic appearance, investigated by Savart. I have some photographs of jets of that nature. Taken as described, they do not differ much in appearance from those obtained by Chichester Bell, and by Mr. Boys. We get what we may regard as simply shadows of the jet obtained by instantaneous illumination; so that these photographs show little more than the outlines of the subject. They show a little more, on account of the lens-like action of the cylinder and of the drops. Here we have an instantaneous view of a jet similar to the one we were looking at just now. [Fig. 2, Plate I.] This is the continuous part; it gradually ripples itself as it comes along; the ripples increase; then the contraction becomes a kind of ligament connecting consecutive drops; the ligament next gives way, and we have the individual drops completely formed. The small points of light are the result of the lens-like action of the drops. [Other instantaneous views also shown.]

The pictures can usually be improved by diffusing somewhat the light of the spark with which they are taken. In front of the ordinary condensing lens of the magic lantern we slide in a piece of ground glass, slightly oiled, and we then get better pictures showing more shading. [Photograph shown.] Here is one done in that way; you would hardly believe it to be water resolved into drops under the action of a tremor. It looks more like mercury. You will notice the long ligament trying to break up into drops on its own account, but not succeeding. [Fig. 3, Plate I.]

There is another, with the ligament extremely prolonged. In this case it sometimes gathers itself into two drops. [Fig. 4, Plate I.]

[A number of photographs showing slight variations were exhibited.]

The mechanical cause of this breaking into drops is, I need hardly remind you, the surface tension or capillary force of the liquid surface. The elongated cylinder is an unstable form, and tends to become alternately swollen and contracted. In speaking on this subject I have often been embarrassed for want of an appropriate word to describe the condition in question. But a few days ago, during a biological discussion, I found that there is a recognised, if not a very pleasant, word. The cylindrical jet may be said to become *varicose*, and the varicosity goes on increasing with time, until eventually it leads to absolute disruption.

There is another class of unstable jets presenting many points of analogy with the capillary ones, and yet in many respects quite distinct from them. I refer to the phenomena of sensitive flames. The flame, however, is not the essential part of the matter, but rather an indicator of what has happened. Any jet of fluid playing into a stationary environment is sensitive, and the most convenient form for our present purpose is a jet of coloured in uncoloured water. In this case we shall use a solution of permanganate of potash playing into

Plate II

Fig 5



Fig 6

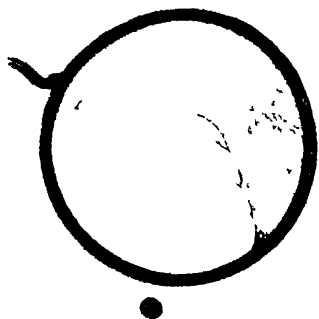


Fig 7

an atmosphere of other water containing acid and sulphate of iron, which exercises a decolourising effect on the permanganate, and so retards the general clouding up of the whole mass by accumulation of colour. [Experiment.] Mr. Gordon will release the clip, and we shall get a jet of permanganate playing into the liquid. If everything were perfectly steady, we might see a line of purple liquid extending to the bottom of the trough; but in this theatre it is almost impossible to get anything steady. The instability to which the jet is subject now manifests itself, and we get a breaking away into clouds something like smoke from chimneys. A heavy tuning fork vibrating at ten to the second acts upon it with great advantage, and regularises the disruption. A little more pressure will increase the instability, and the jet goes suddenly into confusion, although at first, near the nozzle, it is pretty regular.

It may now be asked "What is the jet doing?" That is just the question which the instantaneous method enables us to answer. For this purpose the permanganate which we have used to make the jet visible is not of much service. It is too transparent to the photographic rays, and so it was replaced by bichromate of potash. Here the opposite difficulty arises; for the bichromate is invisible by the yellow light in which the adjustments have to be made. I was eventually reduced to mixing the two materials together, the one serving to render the jet visible to the eye and the other to the photographic plate. Here is an instantaneous picture of such a jet as was before you a moment ago, only under the action of a regular vibrator. It is *sinuous*, turning first in one direction and then in the other. The original cylinder, which is the natural form of the jet as it issues from the nozzle, curves itself gently as it passes along through the water. It thus becomes sinuous, and the amount of the sinuosity increases, until in some cases the consecutive folds come into collision with one another. [Several photographs of sinuous jets were shown, two of which are reproduced in Figs. 5, 6, Plate II.]

The comparison of the two classes of jets is of great interest. There is an analogy as regards the instability, the vibrations caused by disturbance gradually increasing as the distance from the nozzle increases; but there is a great difference as to the nature of the deviation from the equilibrium condition, and as to the kind of force best adapted to bring it about. The one gives way by becoming varicose; the other by becoming sinuous. The only forces capable of producing varicosity are symmetrical forces, which act alike all round. To produce sinuosity, we want exactly the reverse—a force which acts upon the jet transversely and unsymmetrically.

I will now pass on to another subject for instantaneous photography, namely, the soap film. Everybody knows that if you blow a soap bubble it will break—generally before you wish. The process of breaking is exceedingly rapid, and difficult to trace by the unaided eye. If we can get a soap film on this ring, we will project it upon the screen and then break it before your eyes, so as to enable you to form



your own impressions as to the rapidity of the operation. For some time it has been my ambition to photograph a soap bubble in the act of breaking. I was prepared for difficulty, believing that the time occupied was less than the twentieth of a second. But it turns out to be a good deal less even than that. Accordingly the subject is far more difficult to deal with than are those jets of water or coloured liquids, which one can photograph at any moment that the spark happens to come.

There is the film, seen by reflected light. One of the first difficulties we have to contend with is that it is not easy to break the film exactly when we wish. We will drop a shot through it. The shot has gone through, as you see, but it has not broken the film; and when the film is a thick one, you may drop a shot through almost any number of times from a moderate height without producing any effect. You would suppose that the shot in going through would necessarily make a hole, and end the life of the film. The shot goes through, however, without making a hole. The operation can be traced, not very well with a shot, but with a ball of cork stuck on the end of a pin, and pushed through. A dry shot does not readily break the film; and as it was necessary for our purpose to effect the rupture in a well defined manner, here was a difficulty which we had to overcome. We found, after a few trials, that we could get over it by wetting the shot with alcohol.

We will try again with dry shot. Three shots have gone through and nothing has happened. Now we will try one wetted with alcohol, and I expect it will break the film at once. There! It has gone!

The apparatus for executing the photography of a breaking soap film will of necessity be more complicated than before, because we have to time the spark exactly with the breaking of the film. The device I have used is to drop two balls simultaneously, so that one should determine the spark and the other rupture the film. The most obvious plan was to hang iron balls to two electro-magnets, and cause them to drop by breaking the circuit, so that both were let go at the same moment. The method was not quite a success, however, because there was apt to be a little hesitation in letting go the balls. So we adopted another plan. The balls were not held by electro-magnetism but by springs (Fig. 8) pressing laterally, and these were pulled off by electro-magnets. The proper moment for putting down the key and so liberating the balls, is indicated by the tap of the beam of an attracted disc electrometer as it strikes against the upper stop. One falling ball determines the spark, by filling up most of the interval between two fixed ones submitted to the necessary electric pressure. Another ball, or rather shot, wetted with alcohol, is let go at the same moment, and breaks the film on its passage through it. By varying the distances dropped through, the occurrence of one event may be adjusted relatively to the other. The spark which passes to the falling ball is, however, not the one which illuminates the photographic plate. The latter occurs within the lantern, and forms part

of a circuit in connection with the *outer* coatings of the Leyden jars,\* the whole arrangement being similar to that adopted by Prof. Lodge in his experiments upon alternative paths of discharge. Fig. 8 will give a general idea of the disposition of the apparatus. [Several photographs of breaking films were shown upon the screen; one of these is reproduced in Fig. 7, Plate II.]†

This work proved more difficult than I had expected; and the evidence of our photographs supplies the explanation, namely, that the rupture of the film is an extraordinarily rapid operation. It was found that the whole difference between being too early and too late was represented by a displacement of the falling ball, though less than a diameter, viz.  $\frac{1}{2}$  inch nearly. The drop which we gave was about a foot. The speed of the ball would thus be about 100 inches per second; therefore the whole difference between being too soon and too late is represented by  $\frac{1}{300}$  second. Success is impossible, unless the spark can be got to occur within the limits of this short interval.

Prof. Dewar has directed my attention to the fact that Dupré, a good many years ago, calculated the speed of rupture of a film. We know that the energy of the film is in proportion to its area. When a film is partially broken, some of the area is gone, and the corresponding potential energy is expended in generating the velocity of the thickened edge, which bounds the still unbroken portion. The speed, then, at which the edge will go depends upon the thickness of the film. Dupré took a rather extreme case, and calculated a velocity of 32 metres per second. Here, with a greater thickness, our velocity was, perhaps, 16 yards a second, agreeing fairly well with Dupré's theory.

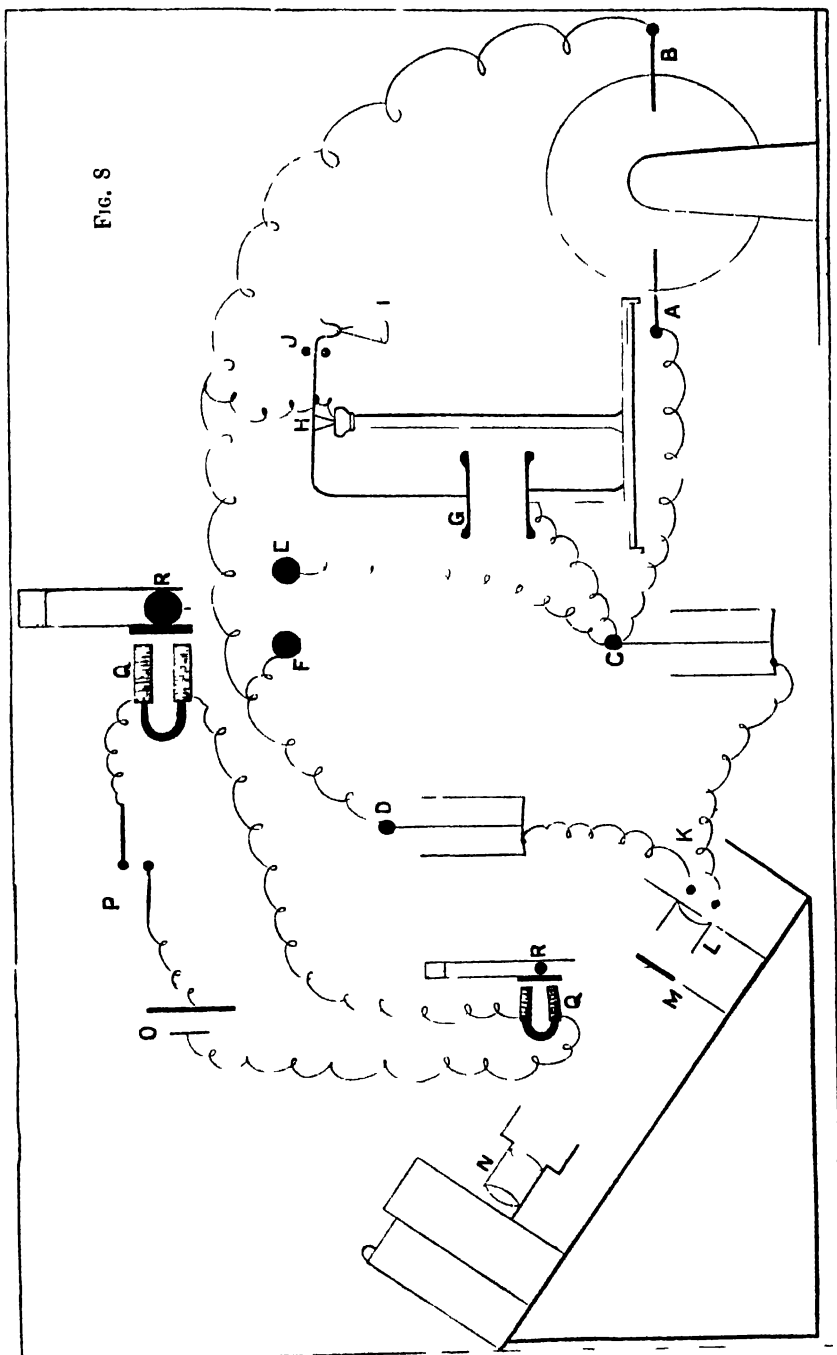
I now pass on to another subject with which I have lately been engaged, namely, the connection between aperture and the definition of optical images. It has long been known to astronomers and to those who study optics that the definition of an optical instrument is proportional to the aperture employed; but I do not think that the theory is as widely appreciated as it should be. I do not know whether, in the presence of my colleague, I may venture to say that I fear the spectroscopists are among the worst sinners in this respect. They constantly speak of the dispersion of their instruments as if that by itself could give any idea of the power employed. You may have a spectroscope of any degree of dispersion, and yet of resolving power insufficient to separate even the D lines. What is the reason of this? Why is it that we cannot get as high a definition as we please with a limited aperture? Some people say that the reason

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\* In practice there were two sets of three jars each.

† The appearance of the breaking bubble, as *seen* under instantaneous illumination, was first described by Marangoni and Stephanelli, *Nuovo Cimento*, 1873.

FIG. 8



why large telescopes are necessary, is, because it is only by their means that we can get enough light. That may be in some cases a sufficient reason, but that it is inadequate in others will be apparent, if we consider the case of the sun. Here we do not want more light, but rather are anxious to get rid of a light already excessive. The principal *raison d'être* of large telescopes, is, that without a large aperture definition is bad, however perfect the lenses may be. In accordance with the historical development of the science of optics, the student is told that the lens collects the rays from one point to a focus at another; but when he has made further advance in the science he finds that this is not so. The truth is that we are in the habit of regarding this subject in a distorted manner. The difficulty is not to explain why optical images are imperfect, no matter how good the lens employed, but rather how it is that they manage to be as good as they are. In reality the optical image of even a mathematical point has a considerable extension; light coming from one point cannot be concentrated into another point by any arrangement. There must be diffusion, and the reason is not hard to see in a general way. Consider what happens at the mathematical focus, where, if anywhere, the light should all be concentrated. At that point all the rays coming from the original radiant point arrive in the same phase. The different paths of the rays are all rendered optically equal, the greater actual distance that some of them have to travel, being compensated for in the case of those which come through the centre by an optical retardation due to the substitution of glass for air; so that all the rays arrive at the same time.\* If we take a point not quite at the mathematical focus but near it, it is obvious that there must be a good deal of light there also. The only reason for any diminution at the second point lies in the discrepancies of phase which now occur; and these can only enter by degrees. Once grant that the image of a mathematical point is a diffused patch of light, and it follows that

## DESCRIPTION OF FIG. 8.

- |  |  |
|--|--|
| A, B, Electrodes of Wimshurst machine.                                   | K, Sparking balls in connection with exterior coatings of jars. [These exterior coatings are to be joined by an imperfect conductor, such as a table.] |
| C, D, Terminals of interior coatings of Leyden jars.                     | L, Lantern condenser.  |
| E, F, Balls on insulating supports between which the discharge is taken. | M, Soap film.  |
| G, Attracted disc of electrometer.                                       | N, Photographic camera.  |
| H, Knife edge.   | O, Daniell cell.   |
| I, Scale pan.  | P, Key.  |
| J, Stops limiting movement of beam.                                      | Q, Electromagnets.   |
|  | R, Balls.  |

\* On this principle we may readily calculate the focal lengths of lenses without use of the law of sines. See 'Phil. Mag.,' Dec. 1879.

there must be a limit to definition. The images of the components of a close double point will overlap; and if the distance between the centres do not exceed the diameter of the representative patches of light, there can be no distinct resolution. Now their diameter varies inversely as the aperture; and thus the resolving power is directly as the aperture.

My object to-night is to show you by actual examples that this is so. I have prepared a series of photographs of a grating consisting of parallel copper wires separated by intervals equal to their own diameter, and such that the distance from centre to centre is  $\frac{1}{10}$ -inch. The grating was backed by a paraffin lamp and large condensing lens; and the photographs were taken in the usual way, except that the lens employed was a telescopic object glass, and was stopped by a screen perforated with a narrow adjustable slit, parallel to the wires.\* In each case the exposure was inversely as the aperture employed. The first [thrown upon the screen], is a picture done by an aperture of eight hundredths of an inch, and the definition is tolerably good. The next, with six hundredths, is rather worse. In the third case, I think that everyone can see that the definition is deteriorating; that was done by an aperture of four hundredths of an inch. The next is one done by an aperture of three hundredths of an inch, and you can see that the lines are getting washed out. In focussing the plate for this photograph, I saw that the lines had entirely disappeared, and I was surprised, on developing the plate, to find them still visible. That was in virtue of the shorter wave-length of the light operative in photography as compared with vision. In the last example, the aperture was only two-and-a-half hundredths of an inch, and the effect of the contraction has been to wash away the image altogether, although, so far as ordinary optical imperfections are concerned, the lens was acting more favourably with the smaller aperture than with the larger ones.

This experiment may be easily made with very simple apparatus; and I have arranged that each one of my audience may be able to repeat it by means of the piece of gauze and perforated card which have been distributed. The piece of gauze should be placed against the window so as to be backed by the sky, or in front of a lamp provided with a ground-glass or opal globe. You then look at the gauze through the pin-holes. Using the smaller hole, and gradually drawing back from the gauze, you will find that you lose definition and ultimately all sight of the wires. That will happen at a distance of about  $4\frac{1}{2}$  feet from the gauze. If, when looking through the smaller hole, you have just lost the wires, you shift the card so as to bring the larger hole into operation, you will see the wires again perfectly.

That is one side of the question. However perfect your lens may be, you cannot get good definition if the aperture is too much

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\* The distance between the grating and the telescope lens was 12 ft. 8 in.

restricted. On the other hand if the aperture is much restricted, then the lens is of no use, and you will get as good an image without it as with it.

I have not time to deal with this matter as I could wish, but I will illustrate it by projecting on the screen the image of a piece of gauze as formed by a narrow aperture parallel to one set of wires. There is no lens whatever between the gauze and the screen. [Experiment.] There is the image—if we can dignify it by such a name—of the gauze as formed by an aperture which is somewhat large. Now, as the aperture is gradually narrowed, we will trace the effect upon the definition of the wires parallel to it. The definition is improving; and now it looks tolerably good. But I will go on, and you will see that the definition will get bad again. Now, the aperture has been further narrowed, and the lines are getting washed out. Again, a little more, and they are gone. Perhaps you may think that the explanation lies in the faintness of the light. We cannot avoid the loss of light which accompanies the contraction of aperture, but to prove that the result is not so to be explained, I will now put in a lens. This will bring the other set of wires into view, and prove that there was plenty of light to enable us to see the first set if the definition had been good enough. Too small an aperture, then, is as bad as one which is too large; and if the aperture is sufficiently small, the image is no worse without a lens than with one.

What, then, is the best size of the aperture? That is the important question in dealing with pin-hole photography. It was first considered by Prof. Petzval, of Vienna, and he arrived at the result indicated by the formula,  $2r' = f\lambda$ , where  $2r'$  is the diameter of the aperture,  $\lambda$  the wave-length of light, and  $f$  the focal length, or rather simply the distance between the aperture and the screen upon which the image is formed.

His reasoning, however, though ingenious, is not sound, regarded as an attempt at an accurate solution of the question. In fact it is only lately that the mathematical problem of the diffraction of light by circular holes has been sufficiently worked out to enable the question to be solved. The mathematician to whom we owe this achievement is Prof. Lommel. I have adapted his results to the problem of pin-hole photography. [A series of curves\* were shown, exhibiting to the eye the distribution of illumination in the images obtainable with various apertures.] The general conclusion is that the hole may advantageously be enlarged beyond that given by Petzval's rule. A suitable radius is  $r = \sqrt{f\lambda}$ .

I will not detain you further than just to show you one application of pin-hole photography on a different scale from the usual. The definition improves as the aperture increases; but in the absence of a lens the augmented aperture entails a greatly extended focal length.

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\* 'Phil. Mag.' Feb 1891.

The limits of an ordinary portable camera are thus soon passed. The original of the transparency now to be thrown upon the screen was taken in an ordinary room, carefully darkened. The aperture (in the shutter) was  $\cdot 07$  inch, and the distance of the  $12 \times 10$  plate from the aperture was 7 feet. The resulting picture of a group of cedars shows nearly as much detail as could be seen direct from the place in question.

[R.]

Friday, March 6, 1891.

WILLIAM CROOKES, Esq. F.R.S. Vice-President, in the Chair.

PROFESSOR J. A. FLEMING, M.A. D.Sc. M.R.I.

*Electro-magnetic Repulsion.*

§ 1. ON the 2nd day of October, 1820, Ampère presented to the Royal Academy of Sciences in Paris an important memoir, in which he summed up the results of his own and Arago's previous investigations in the new science of electro-magnetism, and crowned that labour by the announcement of his great discovery of the dynamical action between conductors conveying electric currents.\* Respecting that achievement, when developed in its experimental and mathematical completeness, no less a writer than Clerk Maxwell calls it "one of the most brilliant in the history of physical science." Our wonder at what was then accomplished is increased when we remember that hardly more than two months before that date John Christian Oersted had startled the scientific world by the announcement of the discovery of the magnetic qualities of the space near a current-traversed conductor. Oersted called the actions going on around the conductor the "electric conflict," and in his first paper,† in describing the newly-observed facts, he says:—"It is sufficiently evident that the *electric conflict* is not confined to the conductor, but is dispersed pretty widely in the circumjacent space." "We may likewise collect," he adds, "that this conflict performs circles round the wire, for without this condition it seems impossible that one part of the wire when placed below the magnetic needle should drive its pole to the east, and when placed above it to the west." These few words are taken from the original paper which stimulated the philosophic thought of Ampère and his contemporaries, and started into existence a wave of discovery, placing us in possession of the facts which form our starting-point to-night.

§ 2. It will be unnecessary to spend more than a moment or two

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\* 'Mémoire présenté à l'Académie Royale des Sciences le 2 octobre 1820, où se trouve compris le résumé de ce qui avait été lu à la même Académie les 18<sup>me</sup> et 25<sup>me</sup> septembre 1820, sur les effets des courants électriques,' par M. Ampère. See vol. xv. 'Annales de Chimie,' 1820.

† In the 'Annals of Philosophy' for October 1820, vol. xvi. p. 274, is to be found an English translation of Oersted's original Latin essay, dated July 21, 1820, describing his immortal discovery. This paper is entitled "Experiments on the Effect of a Current of Electricity on the Magnetic Needle," by John Christian Oersted, Knight of the Order of Danneborg, Professor of Natural Philosophy in Copenhagen.



in illustrating the now familiar interactions of two circuits traversed by currents in the same or opposite directions. Holding a circular coil traversed by a continuous electric current near to a similar circuit free to move, we are well aware that when the circuits are parallel to each other there is an attractive force between them if the currents in adjacent parts of the circuits flow in the same direction, and a repulsive effect if they flow in the opposite. This is the electro-dynamic action discovered by Ampère and utilised in the construction of instruments for the measurement of electric currents in practical work. If one such conducting coil, such as that of the electro-magnet now before me, is traversed by an alternating current, and the other is simply a closed circuit or coil placed a little distance off, but in its field, I feel sure I am free to assume that all here are aware of the effect which will be produced. The closed circuit becomes the seat of an alternating induced current, which, if our inducing current is sufficiently powerful, can be made to render itself evident by illuminating a small incandescent lamp placed in the secondary circuit.\* Notice, however, that in performing the experiment the secondary circuit must be so placed that the magnetic lines of force of the primary coil perforate through the secondary circuit. If the secondary circuit is held in such a position that the reversal of direction of the primary current causes no reversal of direction of the magnetic field traversing the secondary circuit, because it is not linked with any of the lines of force of the primary, the secondary circuit is no longer the seat of any induced current.

Note also that this electro-magnetic induction, whilst taking place across space, is not interfered with by the interposition of a non-conducting screen. The magnetic induction passes freely through an inch board or a plate of glass, but if we interpose a thick copper sheet (Fig 1), we cut off the secondary current or screen the secondary circuit from the inductive action of the primary. The secondary coil is, in fact, an electro-magnetic eye which can *see through* an inch of wood, but to which a sheet of zinc is semi-transparent, and a thick sheet of copper quite opaque, as far as the electro-magnetic radiation of the wave-length employed is concerned. The rapid heating of this copper screen makes us aware that the secondary currents are now being induced in the copper sheet in the form of

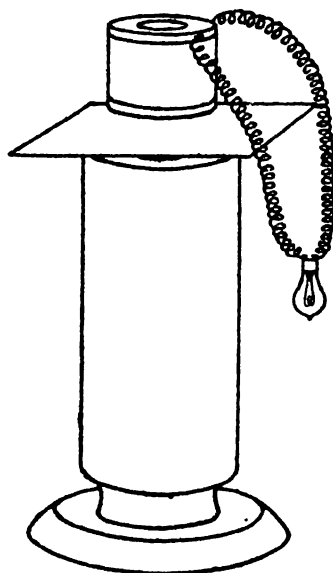
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\* The experiments here referred to were mostly performed with an alternating-current magnet, having a core of divided iron about 3 inches in diameter and 12 inches long, excited by a Siemens alternating-current dynamo, kindly lent for the occasion by Messrs. Siemens Bros., giving a current at an electromotive force of rather less than 200 volts. A small shelf around the core a little above the middle serves as a support for rings, &c., to be projected. The performance of these experiments on a scale suitable for large audiences requires from 10 to 15 horse-power at least, and can hardly be shown well unless the alternator can provide a current of 100 amperes at 100 volts, available at the moment of maximum demand. The magnet of course takes very little actual current until the metal plate or ring is held near it, when the impedance is immediately reduced.

eddy currents, and it screens the secondary circuit because the inductive action of these eddy currents on the side remote from the magnet is exactly equal and opposite to that of the primary circuit on the secondary coil.

§ 3. Connecting together these elementary facts, we are easily able to explain another well-known fact. If a continuous current is sent

FIG. 1.



Copper plate interposed between a primary and a secondary coil and shielding the secondary from induction.

through the coils of a large electro-magnet, and we magnetise its iron core very powerfully, it is found to be impossible to strike the pole of the magnet a sharp blow by means of a sheet of copper. Holding a sheet of copper over such a magnetic pole, and energising the magnet, the hand holding the copper sheet feels a repulsive action at the moment when the current is put on and an attractive action when it is cut off. If we try to slap the magnet sharply with the copper sheet, it is found that this repulsive force prevents anything like such a sharp blow being given to the pole when the current is on, as can be given when the current is off. Moreover, when a very powerful electro-magnet is employed, it is found that a disc of copper let fall over the pole does not fall down sharply and quickly on to it when the current is flowing through the coils of the magnet, but settles down softly and slowly, as if falling through some sticky fluid. We know that the correct explanation of these facts is to be found in the

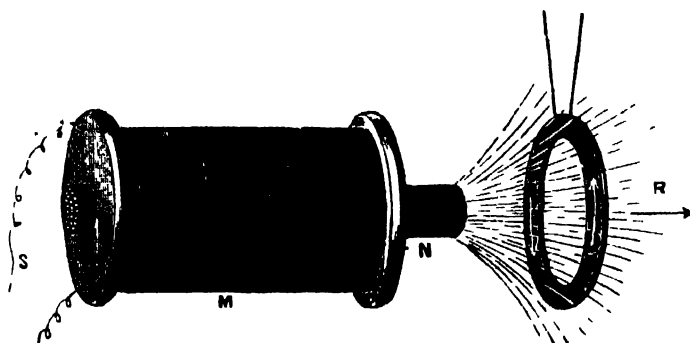
statement that the motion of the conductor towards the magnetic pole causes eddy electric currents to be generated in it by electro-magnetic induction, and that these being in the opposite direction to the energising current of the magnet, cause a repulsive force to exist between the inducing and secondary circuits, which creates the apparent resistance we feel.

In order to exhibit the stress brought into existence between an electro-magnet and a metal sheet held near it when induced currents are set up in the disc, we have arranged the following experiment:—Over the pole of a powerful electro-magnet we have balanced a small disc of copper, the size of a penny, carried on one end of a delicately balanced bar. A mirror attached to that bar serves to reflect on to the screen a ray of light indicating the smallest motion of the copper piece. On magnetising the magnet the copper is suddenly repelled, but comes to rest again immediately in its initial position.

When the magnet is demagnetised the copper experiences a momentary attraction. These attractions and repulsions are obviously due to the Ampèrian stress set up between the magnet and the metal by reason of the induced currents set up in the latter. Impulsive effects of this nature have been particularly studied by Mr. Boys.\*

§ 4. Let me, in the next place, ask you to contemplate a circuit, say a closed conducting ring, suspended in front of the pole of an electro-magnet, and that the coils of the electro-magnet are traversed by an alternating current of electricity (Fig. 2). The magnetic field of the magnet is then an alternating field. We shall suppose it to vary in strength, according to a simple periodic law. The closed circuit is therefore subjected to an inductive action, and we know that the induced electromotive force in that circuit is at any instant propor-

FIG. 2.



Copper ring hung in the field of an electro-magnet, and repelled or attracted when the current is put on or cut off.

tional to the rate of change of the magnetic field in which it is immersed. If, therefore, the variation in strength of that field is represented geometrically by the ordinates of a periodic curve, the varying electromotive force acting in the ring circuit is represented by the ordinates of another such curve of equal wave length, shifted a quarter of a wave length behind the first. In the diagram before you the variation of the induced magnetic field, and the induced electromotive force in the circuit, are represented, as usual, by two harmonic curves. This induced electromotive force creates an induced current flowing backwards and forwards in the ring, and we shall, in the first instance, suppose that this current flows in exact synchronism with its electromotive force. The induced current and the inducing magnetic field may therefore be represented as to relative phase and strength by the curves in the diagram (Fig. 3). The dynamical action,

\* See Proc. Phys. Soc. of London, vol. vi. p. 218, "A Magneto-electric Phenomenon."

or the force which the ring experiences, is at any instant proportional to the product of the strength of the magnetic field in which the ring is immersed, and to the strength of the induced current created in it. If we multiply together the numerical values of the ordinates of these two curves at any and every point on the horizontal line, and set up a new ordinate at that point representing this product, the extremities of these last ordinates define a curve, which is a curve representing the *force* acting on the secondary circuit, and it is, as you see from the diagram, Fig. 3, a wavy curve having a wave length equal to half that of the first two curves. Moreover, the whole area inclosed between the outline of this force curve and the horizontal line represents to a certain scale the time integral of that force, or

FIG. 3.

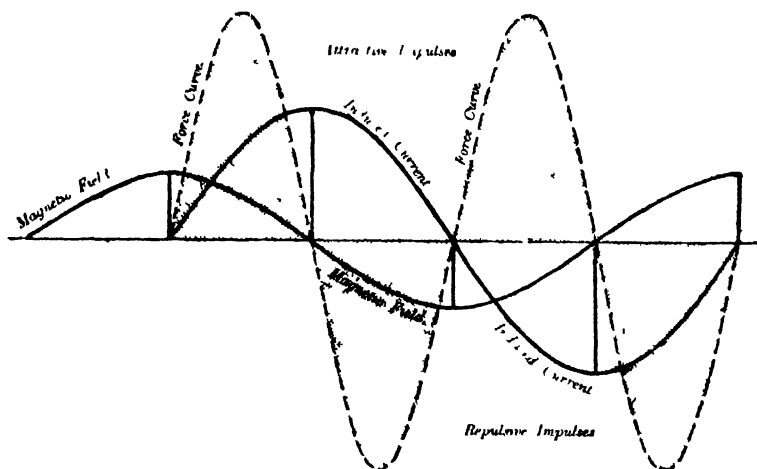


Diagram showing the equality of the attractive and repulsive impulses in a non-inductive circuit when held in an alternating magnetic field.

the *impulse* acting on the secondary circuit, and the theory shows us that, under the assumptions made, the secondary circuit so acted upon experiences in each period of the current four impulses, two positive or repulsive, and two negative or attractive. Hence, it comes to this, that such an ideal conducting circuit held in front of an alternating electro-magnet should experience a rapid alternate series of equal pushes and pulls, or of little impulses to and from the magnet. These equal and opposite impulses in quick succession would neutralise one another, and our supposed circuit would not, on the whole, be subject to any resultant force.

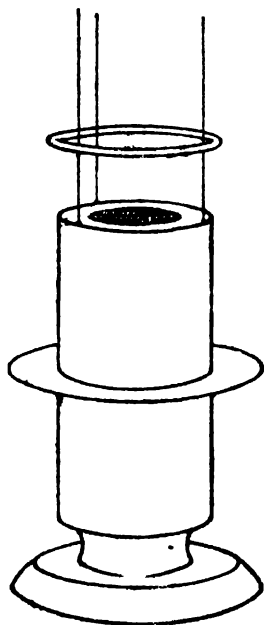
§ 5. Quite otherwise is it, however, when we present a real conducting circuit to the pole of an electro-magnet traversed by a powerful alternating current. We find that under the actual circumstances there is a powerful repulsive action between the pole and the

circuit. The exact nature of the electro-magnetic repulsion it is our business now to explore.

Taking in my hands a copper ring, I hold it over the pole of this powerful vertical alternating electro-magnet, and find at once that there is a perceptible and strong repulsion. Letting the ring go, it jumps up into the air, impelled so to do by the electro-magnetic repulsion acting upon it (Fig. 4). All good conducting rings will execute this gymnastic feat, and rings of copper and aluminium are found to be most nimble of all. Rings of zinc and brass are sluggish, and a ring of lead will not jump at all. We owe the discovery of this striking effect to Prof. Elihu Thomson, and he has explored in all directions the consequences and nature of this interesting effect. Why is it that our real copper and metal rings behave so differently, when immersed in an alternating field, to ideal rings of conducting matter? The explanation is not very difficult to find. The real ring possesses a quality, called its inductance, of which we took no account in our examination of the case a moment ago.

§ 6. Before me lies a very large bobbin of wire (Fig. 5), and the ends of this coil are connected to an incandescent lamp. We send a current of electricity through the bobbin and the lamp, and have arranged matters as in the diagram before you, so that the current divides through the two circuits of lamp and bobbin. Under these circumstances, the divided current is just sufficient to illuminate faintly the lamp. I break the circuit of the battery, and you see that the lamp flashes up for a moment, and we are well aware that this effect is due to the electro-magnetic momentum of the coil, or to its inductance, in virtue of which the current continues to flow on in the bobbin for a short period after the impressed electromotive force is withdrawn. Also, we know that there is a small but definite time required before the current practically reaches its full strength in the bobbin when the electromotive force is again applied. These effects are the self-inductive effects of a coil first noticed by Prof. Joseph Henry in 1832, and subsequently fully examined in the ninth series of Faraday's 'Experimental Researches in Electricity.' Every circuit of any kind—disc, ring, or coil of wire—possesses a certain degree of this quality of self-induction, and, as a natural consequence it follows therefrom that if that circuit is subjected to a periodically varying electro-

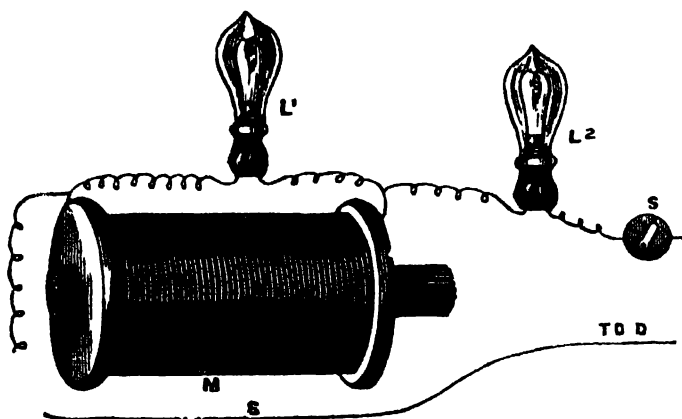
FIG. 4.



Aluminium ring projecting from the pole of an alternating electro-magnet, and floating over the pole when restrained by three strings.

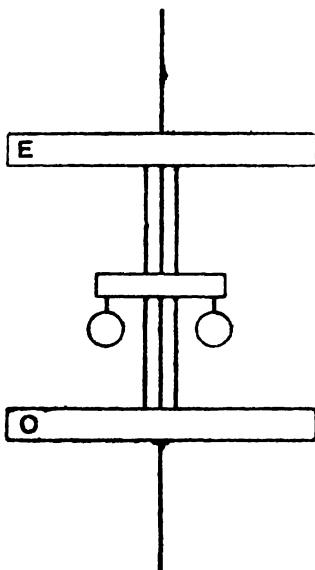
motive force acting upon it, the current produced in it will always be in arrear of the electromotive force in phase. The current

FIG. 5.



Incandescent lamp and coil arranged to exhibit the effect of the inductance of the coil.

FIG. 6



Mechanical model illustrating the "lag" of the current in an inductive circuit.

induced lags in phase behind the inducing electromotive force. This "lag" of the current behind the impressed harmonically varying electromotive force in consequence of self-induction

may be illustrated mechanically thus:—Three light wooden laths are connected together by a flat steel band, and the system hung up by a string like a pair of astatic magnetic needles (Fig. 6). If we take hold of the upper rod and move it backwards and forwards on a horizontal plane, compelling it to execute harmonic oscillations, the upper and lower rods move together synchronously. Let the upper rod symbolise impressed electromotive force, and the lower rod resulting current. Under present circumstances, these two move always in step with one another. Next load down the middle rod or connecting mechanism by means of two lead weights, and give it *inertia*. On again taking hold of the upper rod and causing it to

FIG. 7.

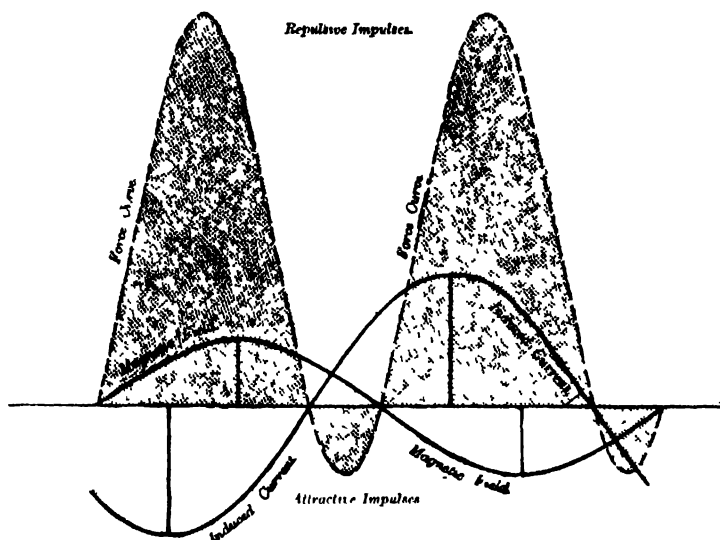


Diagram showing the inequality of the attractive and repulsive impulses in the case of an inductive circuit when held in an alternating magnetic field.

execute forced harmonic oscillations, the lower rod no longer vibrates in tune with it. It lags in phase behind the top one, and we thus illustrate, though not perhaps by a perfect mechanical analogy, the effect of electric inertia in the intermediate mechanism in causing a lag of current behind impressed electromotive force.

§ 7. Returning for an instant to the diagram we considered just now, we must correct it to make it fit in with the facts of nature, and we must represent the periodic curve which stands for the fluctuations of the induced current in the ring as shifted backwards or lagging behind the curve which represents the electromotive force in the circuit brought into existence by the fluctuating magnetic field. Making this change in our diagram (Fig. 7), and forming as before a force curve to represent the impulses on the ring, we now find that owing to the 'lag' of the secondary current, one set of the impulses,

namely, the positive or repulsive impulses, have been enlarged at the expense of the negative or attractive impulses. Theory, therefore, points out that as a consequence of the self-induction of the ring the balance between the attractive impulses and repulsive impulses is upset, and that the latter predominate. Our real ring behaves, therefore, very differently to our ideal ring. The real ring is strongly repelled, because the resultant action of all the impulses is to produce on the whole an electro-magnetic repulsion. This repulsion is evidence of the self-induction of the circuit exposed to the magnetic field, and it forms a new way of detecting it. But although this is part of the truth, it is not the whole truth. The lag of the induced current in the ring, and hence the predominance of the repulsive impulses, depends on the conductivity of the material of which the ring or circuit is made; and the better this conductivity the greater is that repulsion, because both the induced current and the "lag" are thereby increased. Hence it comes to pass that there are two factors involved in making this repulsive effect, the conductance of the ring or disc and its inductance. For equal conductivities, the greater the self-induction, the greater the repulsion. For equal self-inductions, the greater the conductivity of the circuit so much the more repulsive effect will be produced. Time does not permit me to enlarge on the strict analysis of the effect. Its broad outlines are indicated, perhaps, sufficiently, by what has been said.

§ 8. We can show the effect of the relative conductivity of discs of equal size, and therefore of equal self-induction, by *weighing* similar discs of various metals over an alternating pole. Here, for instance, are discs of copper, zinc, and brass of equal form and size. Placing these discs on the scale pan of a balance, and suspending them over the alternating pole, I am able to prove that the repulsion on the copper disc is greater than that on the zinc, and that on the zinc greater than that on the brass.\*

The same result can be illustrated by placing over the pole of our alternating magnet a paper tube. Taking one of the copper rings in my hand, and first exciting the magnet, I let the ring drop down the tube. It falls as if on an invisible cushion that buoys it up, and it remains floating in the air. If rings of different metals and equal size are placed on the tube, they float at different levels like various specific-gravity beads in a liquid. The greater the conductivity of the ring, the greater is the repulsion on it, in any given part of the alternating field, and hence the highly conducting rings will be sustained on a weaker field than the feebler conducting rings, assuming the rings to have about equal weights. Moreover, we are able to show by another experiment the fact that these rings are traversed, when so held, by powerful electric currents. If we press

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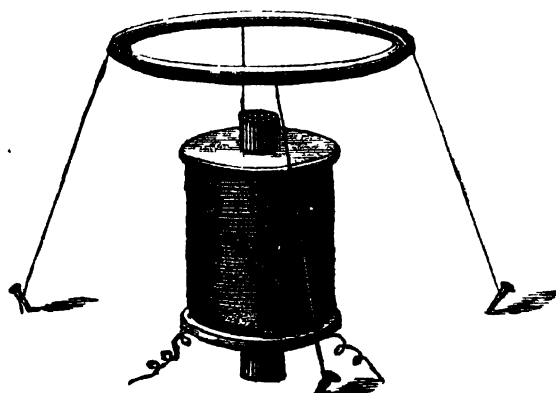
\* Experiments of this kind have been made by M. Borgman. See 'Comptes Rendus,' No. 16, April 21, 1890, p. 849; and also February 3, 1890, vol. cx. p. 233.



down the copper ring upon the zinc or brass ring floating beneath it, the rings are attracted together and the copper ring holds up the zinc. This is obviously because the rings are all traversed by induced currents circulating in the same direction.

§ 9. It is, of course, an immediately obvious corollary, from all that has just been said, that any cutting of a ring or disc which hinders the flow of the induced currents causes the whole of the repulsion effects to vanish. We illustrate this by causing a ring of copper wire to jump off the pole, and then cutting it with pliers, find it has ceased to be capable of giving signs of life. When the metallic masses or circuits which are presented to the alternating magnetic pole are of very low resistance, the electro-magnetic repulsion may become very powerful, many pounds of thrust or push being produced

FIG. 8.



Copper ring "floating" in air over the pole of an alternating-current electro-magnet, when restrained by strings.

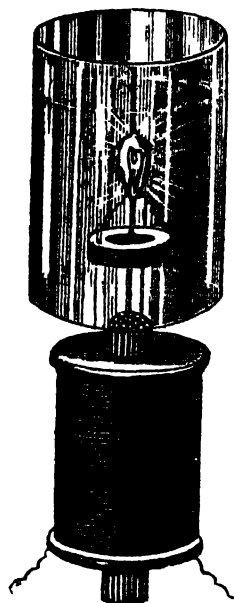
by apparatus of quite moderate size. It is, in fact, quite startling to hold over the pole of such an alternating magnet as I have before me a very thick plate of high conductivity copper. It would greatly surprise any one not acquainted with these principles to be told that a massive copper ring weighing eight or ten pounds could be made to float in the air, but you have ocular demonstration before you that this feat can be performed. The ring needs to be tethered by light strings, to prevent it from being thrown off laterally, although these strings in no way support its weight (Fig. 8).

One of the most beautiful of Prof. Elihu Thomson's experiments exhibits this effect of electro-magnetic repulsion on a closed coil, which is buoyed up in water by a small incandescent lamp in circuit therewith. In the glass vase before you floats a little glow-lamp like a balloon (Fig. 9). The car consists of a coil of insulated wire, and the ends of this coil are connected with the lamp. The whole arrangement is accurately adjusted to just, or only just, float in water.

Placing the vase over an alternating magnetic pole, you see that the magnetic induction creates a current in the coil which lights the lamp, and, moreover, that the electro-magnetic repulsion on the coil causes the lamp and coil to rise upward in the water.

§ 10. We must now pass on to study shortly another class of actions, namely, deflections and rotations produced by electro-magnetic

FIG. 9.



Incandescent lamp and secondary coil floating in water and repelled by an alternating-current electro-magnet, placed beneath.

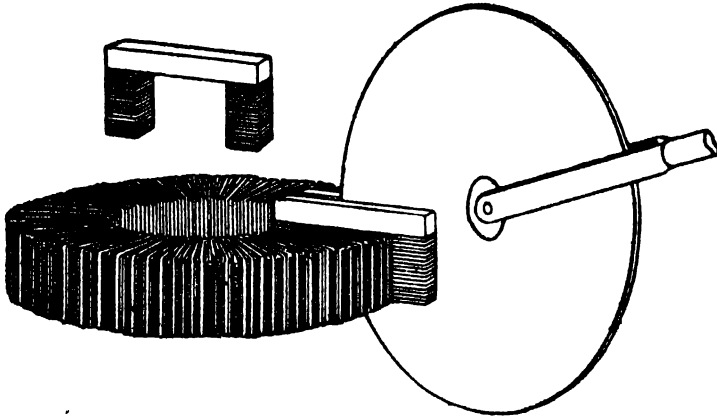
repulsion on highly conducting discs or rings. If the conducting ring or disc which is presented to the alternating pole is constrained by being fixed to an axis around which it can rotate, the action may reduce to a defective force. Here, for instance, is a flat disc fixed on a transverse axis. On presenting this disc to the pole, the disc is prevented by its constraint from being repelled bodily; so it does the next best thing it can, it sets its plane parallel to the lines of magnetic force, and gets into such a position that the induced currents in it are reduced to a minimum. On this principle, before becoming acquainted with Prof. Elihu Thomson's original work, I devised a little copper disc galvanometer for detecting small alternating currents.

§ 11. More interesting than the defective actions are those which result in the production of continuous rotation in highly conducting bodies placed in an alternating field. This electro-magnet in front of me, and which has come from Prof. Elihu Thomson for the purposes of this lecture, consists, as you see, of a nearly closed circuit divided iron core wound over with a coil (Fig. 10). The ends of the iron circuit are provided with copper bars, which embrace and cover portions of the polar terminations of the magnet. When the magnet is excited by a periodic current, these secondary

circuits become the seat of powerful induced secondary currents. Taking in hand a large copper disc pivoted at the centre and held in a fork, we hold this wheel so that part of the disc is inserted between the jaws of the electro-magnet. Immediately, rapid rotation is produced. The reason is not far to seek. The alternating field induces, both in the closed coils and in the neighbouring portions of the disc, induced currents; these tend to cause the parts of the conductors in which they flow to be pulled into parallelism, and if the polar coils are so placed as to partly shield the poles these attractive actions act unsymmetrically on the disc and pull it continuously round. The action is, perhaps, better illustrated by a simpler experiment. If we hold a pivoted copper disc (Fig. 11) symme-

trically over an alternating pole, the action of the pole is one of pure repulsion on the disc, which, however, causes no rotation in it.

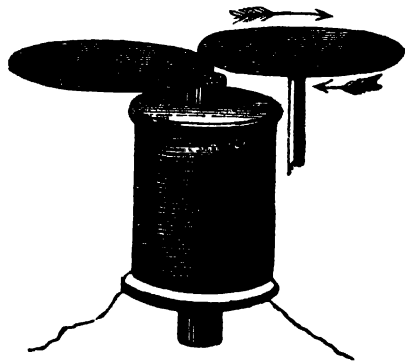
FIG. 10.



Electro-magnet with shaded poles causing a copper disc placed between the jaws to revolve.

When a copper sheet is so placed as to shield or "shade," as Prof. Thomson calls it, part of the magnetic pole, currents are induced both in the fixed plate and in the movable one. The fixed disc shields part of the other from the induction of the pole, and hence causes the induced currents in that plate and disc to be so located that they are in positions to cause continual attraction between one another and continuously pull round the movable disc into fresh positions, so creating regular rotation. This principle of "shading" a pole is employed in constructing the polar coils of the magnet used in our experiment a moment ago, and the experiments present us with a form of self-starting alternating-current motor, although not perhaps a very efficient one in the technical sense. This principle of "shading" a portion of a conductor from the inductive action of the pole, and so causing the eddy currents in it to be located in a portion of its surface and to

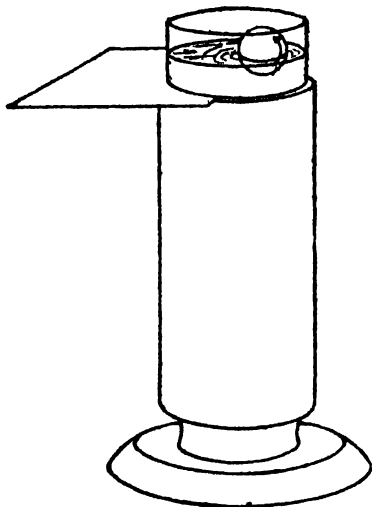
FIG. 11.



Revolution of a shaded copper plate held over an alternate-current magnetic pole.

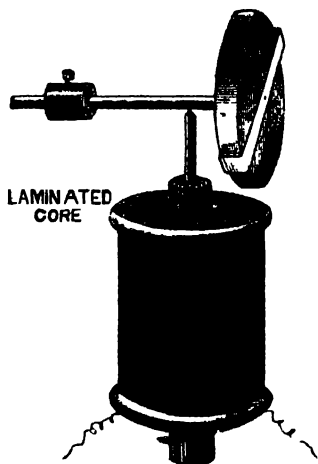
cause attraction between that conductor and the shading conductor is capable of being exhibited in various ways. We place on this copper plate a light hollow copper ball, and support it in a little depression in a copper plate. Holding the arrangement over the alternating magnet, the ball begins to spin round rapidly when the magnet is excited. This rotation is caused by the continual attraction of the eddy currents induced in the fixed plate and in that part of the ball which is not shielded from the pole by the plate. We may vary the experiment, and exhibit many more or less curious and amusing illustrations of it. If we float these copper balls in water (Fig. 12),

FIG. 12.



Hollow copper ball floating in water over an alternate current electro-magnet, and caused to revolve by the interposition of a "shading" plate.

FIG. 13.



Electro-magnetic gyroscope revolving over the pole of an alternate-current electro-magnet.

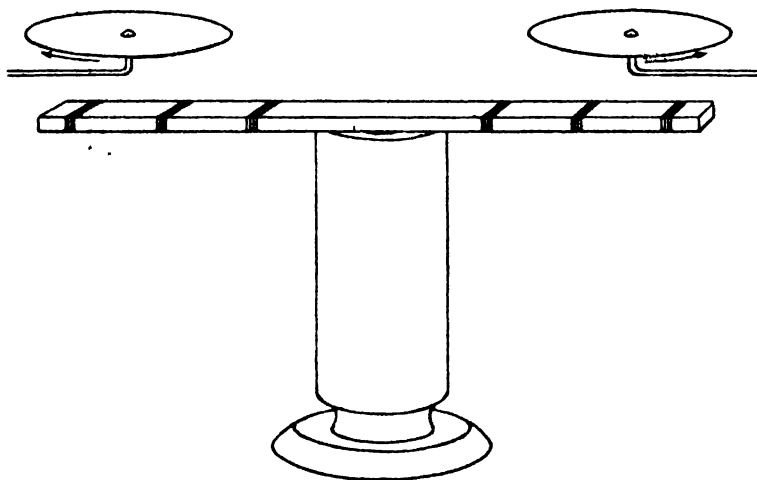
and place the glass bowl containing them over the alternating pole, the interposition of a copper sheet between the pole and the balls causes the latter to begin to spin in a highly energetic manner.

§ 12. Prof. Elihu Thomson has invented a novel form of electro-magnetic gyroscope (Fig. 13). We have now suspended over the alternating magnet a gyroscope of the usual form. The wheel of the gyroscope is made of iron, and the tyre of the wheel is a thick copper band. Immediately the magnet is energised, the gyroscope begins to rotate with great rapidity over the pole. In this case the unsymmetrical disposition of the eddy currents in the copper band around the wheel is sufficient by itself to cause the rotation to occur. The phenomenon which, however, lies at the bottom of all these effects is that the self-induction of the secondary circuit causes the

eddy currents to be delayed in phase behind the magnetising field, and hence to persist into the period of reversal of that field, and so produce the repulsion between the primary conducting circuit and that part of the secondary conducting circuit in which the eddy currents are set up.

One more experiment in this part of the subject, before we pass on to some other developments of it, shall be placed under your notice. Returning to the use of the electro-magnet, in which the iron circuit is all but complete, we find that when a highly-conducting disc is put between the closely approximated half-shielded jaws of this electro-magnet, and an alternating current employed to excite it, the conducting disc is held up in the air-gap

FIG. 14.



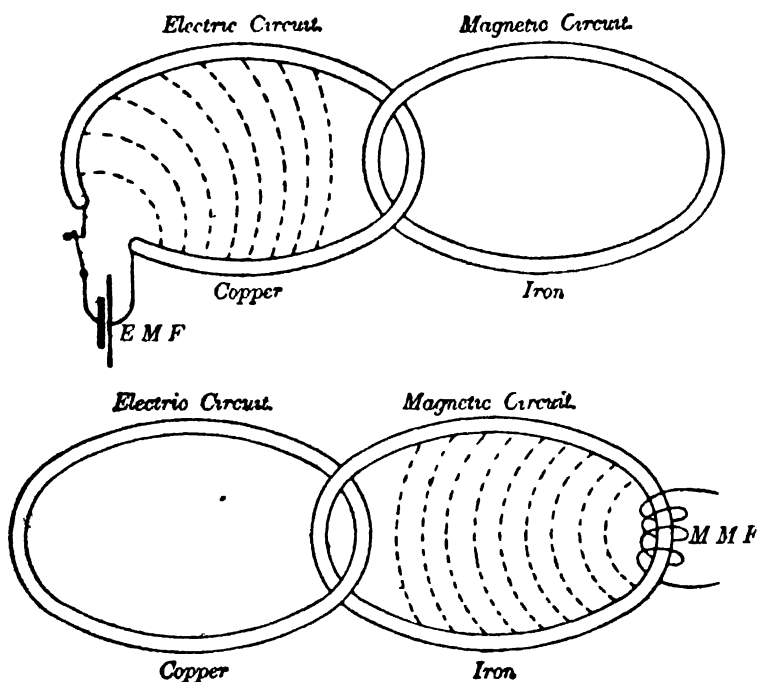
Alternating magnetised iron bar causing revolution of two iron discs held near its extremities.

by reason of the electro-magnetic attraction set up between the disc and the shielding polar plates. If, however, the disc has a relatively poor conductivity, the attraction is not nearly so marked. A good or bad silver coin can be discriminated thereby, because the good silver coin has conductivity enough to be the seat of powerful induced currents, but the bad coin has not.

§ 13. Closely akin to the foregoing, but rather less easy to explain, are the rotations in copper and iron discs which can be caused by the approximation to them of a laminated iron bar alternately magnetised. These actions have been carefully studied by Prof. Elihu Thomson, and applied by him and others in many practical devices. Across the top of this electro-magnet we place a long bar of laminated iron with the plane of the lamination vertical (Fig. 14). This bar is throttled at intervals by copper bands, which

form small closed secondary circuits upon it. We excite the magnet, and hold near the bar an iron disc capable of free rotation ; it begins to rotate rapidly, as you now see. Not only can this be done with a laminated bar throttled by conducting circuits, but even a solid bar of hard steel will serve the same purpose, and a couple of steel files placed across the poles can cause rapid rotation in pivoted discs of copper or of iron held with their edges close to the bars so alternately magnetised. To elucidate this remarkable action, we must revert for a moment to some fundamental facts. Here are two paper rings interlinked, one of red, the other of blue paper (Fig. 15). Let the red

FIG. 15



Diagrams illustrating the symmetry in relation between electromotive force and electric current, and magnetomotive force and magnetic induction.

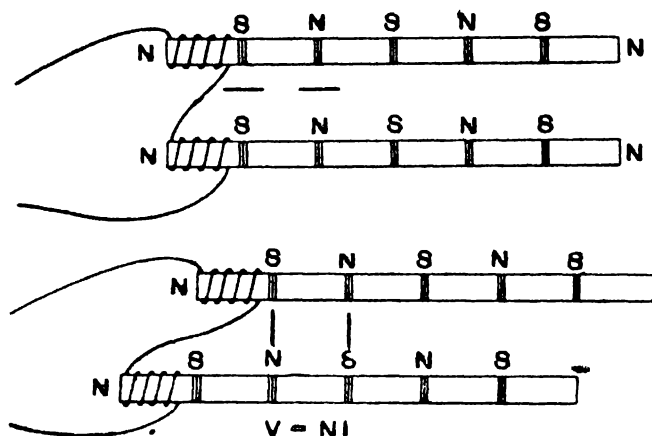
ring stand as a symbol for a copper or conductive circuit. Let the blue ring stand for an iron or magnetic circuit. If we introduce into the conductive circuit an impulsive or alternating *electromotive force*, we are well aware that the interlinked iron circuit, by increasing the self-induction of the conductive circuit, hinders the change of current strength in it by introducing a *back electromotive force* of self-induction. Consider now the iron circuit. If we introduce into that magnetic circuit an impulsive or alternating *magnetomotive force* by interlinking it with some turns of a magnetising

current, the effect of the copper or conductive circuit, which is linked with the iron or magnetic current, is similarly to introduce a *back magnetomotive force* into the magnetic circuit by reason of the magnetic field set up by the secondary current generated in that copper or conducting circuit. In other words, the secondary current induced in the copper circuit by any change in the magnetomotive in the iron circuit is in such a direction that it operates to oppose that primary magnetomotive force, chiefly, however, at the spot where the copper circuit passes round the iron. The general result may be stated to be that the action of the interlinked copper circuit is to cause the magnetic induction in the iron circuit to leak across through the air and partly to escape, passing through the secondary circuit. This escape of induction is called magnetic leakage, and the induced current set up in the closed secondary circuit is the cause of this magnetic leakage. There is a symmetry in the relations of magnetomotive force and the magnetic induction and electromotive force and electric current, and we can, as Faraday pointed out long ago, make the symmetry complete, if we suppose the two interlinked magnetic and electric circuits immersed in an imperfectly conducting medium. If then, we throttle a magnetic circuit, such as a laminated iron bar with copper coils closed upon themselves, and place a magnetising coil at one end, the closed conducting circuits hinder the rise of magnetic induction in the bar; in other words, they give it what may be called *magnetic self-induction*. If the source of magnetism is a rapidly-reversed pole, the consequences of this delay or "lag" in the induction is that a series of alternating magnetic poles are always travelling with retarded speed up the bar, and these may be considered to be represented by tufts of lines of magnetic force which spring out from and move laterally up the bar. If the bar is not laminated and not throttled, the eddy currents set up in the mass of the bar itself act in the same way, and operate to resist the rise of induction in the bar and to delay the propagation of magnetism along it. Hence we must think of such a throttled bar, when embraced by a magnetising coil at one end, as surrounded by laterally moving bunches of lines of magnetic force, which move up the bar. Each reversal of current in the magnetising coil calls into existence a fresh magnetic pole at the one end of the bar, which is, as it were, pushed along the bar to make room for the pole of opposite name, which appears the next instant behind it. When an iron disc is held near such a laminated and throttled bar, these laterally moving lines of force induce poles in the disc which travel after the inducing poles, and hence the disc is continually pulled round. If the disc is a copper disc, the laterally moving lines of magnetic force induce eddy currents in the disc, and these, by the principle already explained, create a repulsion between the pole and the part of the disc in which the eddy currents are set up.

§14. The progression of alternate poles along a bar can be investigated by means of an experiment due to Mr. A. Wright.

Two laminated straight iron bars (Fig. 16) are throttled at intervals with secondary circuits, and have wound on one extremity a magnetising coil. The two bars are placed near each other and parallel. The coils are so connected that the poles at any instant in the ends of the two bars are of similar name. An alternate current is sent through the coils joined in series. Under these circumstances a series of alternate poles of similar names run up the bar parallel with one another. A small, soft iron needle hung at any place between the bars sets itself parallel to the bars, because at any instant poles of similar names are abreast of one another at any spot in the length of the bars. If, however, we shift one bar lengthways backwards or forwards through a certain distance, so as to bring opposite

Fig. 16.



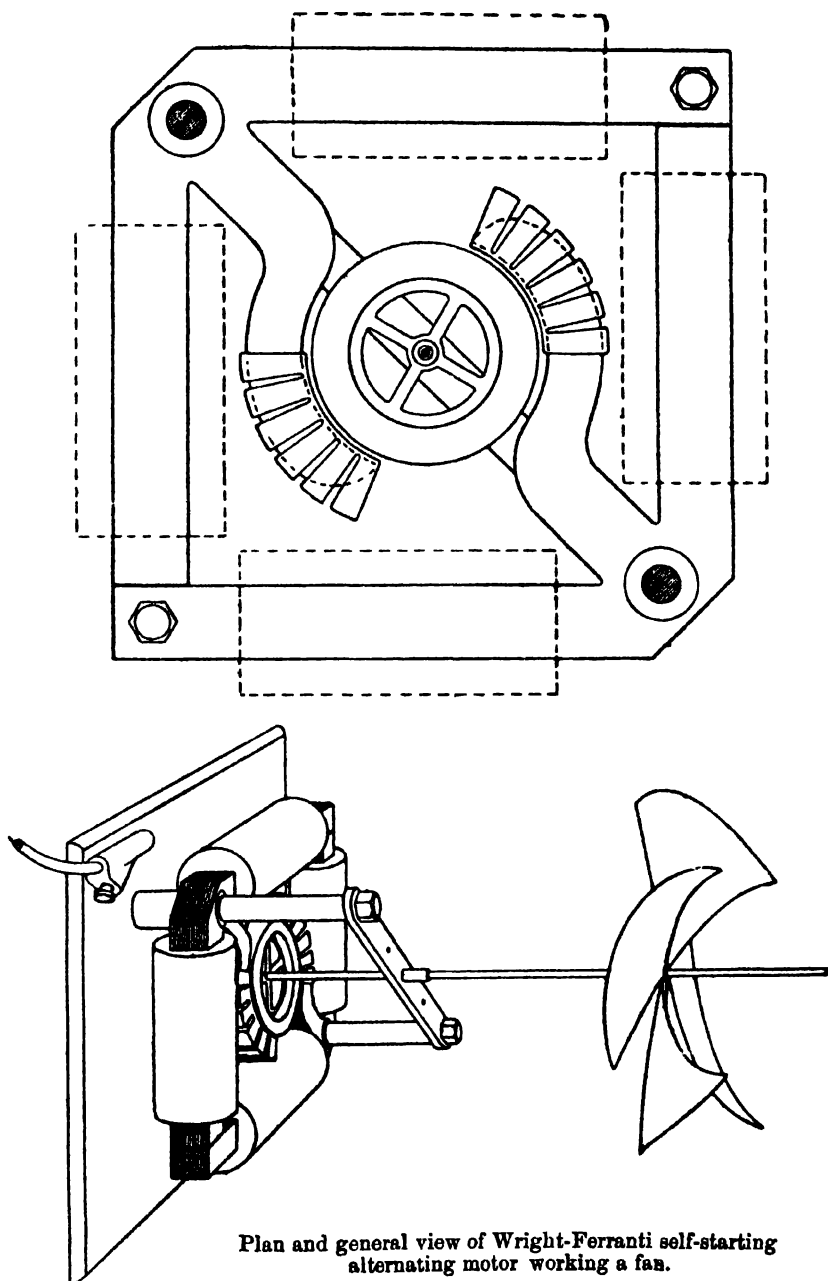
Mr Wright's experiments with throttled and alternately magnetised bars

poles abreast of each other throughout their journey up the bars, we shall find a position such that the soft iron needle will set at right angles to the bars when hung at any point in the space between them. The distance by which we have to shift the one bar backwards of the other to effect the change is evidently half a magnetic wave length, and knowing the frequency of the alternations we can readily arrive at a measure of the velocity of propagation of these alternate poles in the bar. This velocity is evidently numerically equal to the product of the frequency and wave length so obtained.

§ 15. A very pretty application of the above principle has been made in the electric meter of Messrs. Wright and Ferranti for measuring alternating currents. Before me stands one of these meters. It consists of a pair of vertical electro-magnets, with laminated iron cores, and each magnet bears at the top a curved horn of laminated iron which is throttled by copper rings. These curved horns, springing from the magnets, embrace and nearly touch



FIG. 17.

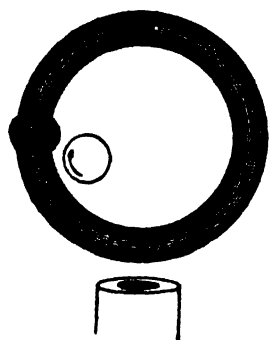


Plan and general view of Wright-Ferranti self-starting alternating motor working a fan.

a light iron-rimmed wheel, free to turn in the centre. The actions just explained drive the wheel round, when the magnet coils are traversed by an alternating current. The iron wheel carries on its shaft a set of mica vanes, which retard the wheel by air friction. Under the opposing influences of this retardation and the electro-magnetic rotation forces, the wheel takes a certain speed corresponding to different current strengths in the magnetic coils, and hence the total number of revolutions of the wheel in a given time, as recorded by a counter, serves to determine the total quantity of alternating current which has passed through the meter. A motor (Fig. 17) working a fan is also here exhibited, the operation of which depends on the same facts. In the case of the motor the iron-rimmed wheel has its tyre closed with copper sheet to aid the action.

§ 16. The rotation of iron discs can be shown also by means of a badly-designed transformer. If a closed laminated iron ring (Fig. 18),

FIG. 18.



Magnetic leakage across a throttled iron ring, causing rotation of an iron disc placed near the secondary coil.

like the one before me, is wound with a couple of conducting circuits, such an arrangement constitutes a transformer. If these two circuits are wound on opposite sides of the iron ring, the previous explanations will enable you to perceive that the arrangement will be productive of great magnetic leakage across the iron circuit. In designing transformers for practical work, one condition amongst others which must be held in view is to so arrange the conductive and magnetic circuits that a great magnetic leakage of lines of force across the air does not take place. If, however, this leakage exists, it indicates that the secondary circuit is not getting the full benefit of the induction created by the primary. To detect it we have merely to hold near the iron circuit a little balanced or pivoted iron disc, and if it is set in rapid rotation, as you observe

in this case, it indicates that there are laterally-moving lines of magnetic force outside the iron, which have escaped from the iron in consequence of the back magneto-force of the secondary circuit.

§ 17. Time would fail me if I were to attempt to enlarge on the practical applications of the scientific principles which these experiments disclose to us. They are a fertile field both for the investigator seeking to add to the sum total of existing knowledge, or to the inventor in search of applications in electrical technology for such acquired facts. Ingenious minds, and that of Prof. Elihu Thomson foremost amongst them, are busy in seeking to turn these facts to account in the construction of alternating current motors.

One of the simplest of these is shown in principle in the diagram now on the screen (Fig. 19). The coils C are traversed by an alter-

nating current, and are placed on either side of a drum armature wound over with three sets of insulated wire coils, the terminals of the coils coming to insulated sections of a commutator. The functions of this commutator are to keep one coil, B, on short circuit during the time when it is in such positions relatively to the field coils C that the induced current in the closed coil causes it to be repelled by the field coils, and as each successive coil on the armature becomes in turn the active coil, rotation is kept up. A motor, made by Prof. Thomson, based on these principles, but with some additions, is on the table, and on turning the current into it it speedily starts and gets up considerable speed. The details of the actual construction are a little less simple than in the diagram shown, because the

FIG. 19.

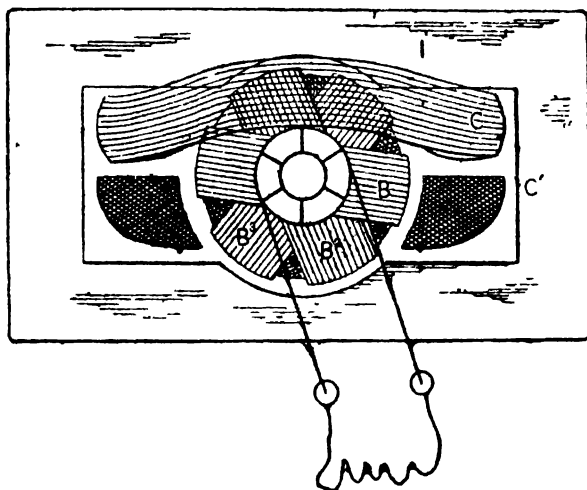


Diagram illustrating elementary form of alternating-current motor.

motor is made to start by sending the external current into the armature by means of a commutator and brushes. When, however, the proper speed is attained, the armature coils are automatically short-circuited, and the motor continues to run in virtue of the current induced by the field-magnet in the armature coils. This is by no means, however, the last word said on this portion of the applications, and I think we may shortly look to Prof. Thomson to give us further and more perfect methods of utilising these facts in the construction of self-starting alternating motors.

§ 18. For the opportunity of exhibiting to you this evening these remarkable experiments, I am personally indebted to Prof. Elihu Thomson, both for constructing and sending the apparatus we have used. Part of these appliances have now an historical value, and have been presented by him to the Royal Institution. For the use

of the rest I desire to record my obligations and thanks. Illustrative as these experiments are of important facts in connection with the use of alternating currents, they have a special value at the present time. In the opening year of next century, when we celebrate the centenary of the first practical production by Volta, in 1801, of the electric current, we shall find ourselves in the presence of the fact that almost every large city is ramified by a subterranean network of copper conductors for the distribution of electric energy as a necessary of modern life; and although it may be dangerous to express too confident a view on the direction which the progress of electrical invention may take, yet it does not seem improbable that the alternating current will be doing a considerable share of that work. It is, therefore, not only as a contribution to a comprehension of the vagaries of these alternating currents that the phenomena we have shortly studied are worthy of attention, but also as being, perhaps, the avenue of approach to a further possession of valuable knowledge, enlarging our views, and capable, without doubt, of being minted into the current coin of useful and ingenious applications.\*

[J. A. F.]

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\* For the loan of the blocks, illustrating the foregoing reprint, the author is indebted to the Editor of the *Electrician*

Friday, April 10, 1891.

WILLIAM CROOKES, Esq. F.R.S. Vice-President, in the Chair.

SIR WILLIAM THOMSON, D.C.L. LL.D. Pres. R.S. M.R.I.

*Electric and Magnetic Screening.*

THERE are five kinds of screening against electric and magnetic influences, which are quite distinct in our primary knowledge of them, but which must all be seen in connected relation with one another when we know more of electricity than we know at present:—I. Electrostatic screening; II. Magnetostatic screening; III. Variational screening against electromotive force; IV. Variational screening against magnetomotive force; V. Fire-screens and window-blinds or shutters.

I.

Electrostatic screening is of fundamental significance throughout electric theory. It has also an important place in the history of Natural Philosophy, inasmuch as consideration of it led Faraday from Snow Harris's crudely approximate but most interestingly suggestive doctrine of non-influence of unopposed parts and action in parallel straight lines between the mutually visible parts of mutually attracting conductors, to his own splendid theory of inductive attraction transmitted along curved lines of force by specific action in and of the medium intervening between the conductors.

A continuous metallic surface completely separating enclosed air from the air surrounding it acts as a perfect screen against all electrostatic influence between electrified bodies in the portions of air so separated. This proposition, which had been established as a theorem of the mathematical theory of electricity by Green, in the ninth article of his now celebrated essay,\* was admirably illustrated by Faraday, by the observations which he made inside the wooden cube covered all around with wire netting and bands of tinfoil, which he insulated within this lecture-room:† “I went into the cube and lived in it; and, using lighted candles, electrometers, and all other tests of electrical states, I could not find the least influence upon them, or indication of anything particular given by them, though all the time the outside of the cube was powerfully charged, and large sparks and brushes were darting off from every point of its outer surface.”

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\* See pp. 14 and 48 of the reprint edited by Ferrers.

† ‘Experimental Researches,’ 1173-1174.

The doctrine of electric images is slightly alluded to, and an illustrative experiment performed, showing the fixing of an electric image. The electroscope used for the experiments is an electrified pith ball, suspended by a varnished double-silk fibre of about 9 or 10 feet long. Figs. 1-4 represent experimental illustrations, in which the pith ball, positively electrified, experiences a force due to electrified bodies, optically screened from it by a thin sheet of tin-plate. In Figs. 1 and 2 the pith ball is attracted round a corner by a stick of rubbed sealing-wax, and in Figs. 3 and 4 repelled round a corner by a stick of rubbed glass. In Fig. 2 the sealing-wax *seems* to repel the pith ball, and in Fig. 4 rubbed glass *seems* to attract it.

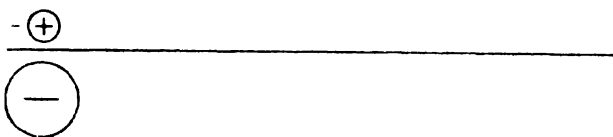


FIG. 1

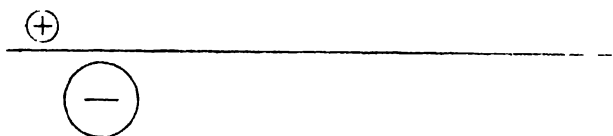


FIG. 2

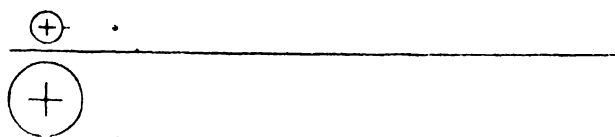


FIG. 3.

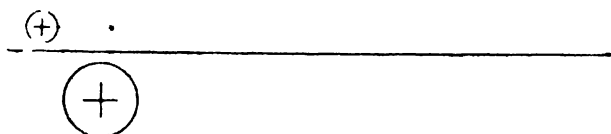


FIG. 4

This experiment constituted a very palpable illustration of Faraday's induction in curved lines of force.

In the present lecture some experimental illustrations were given of electrostatic screening by incomplete plane sheets and curved surfaces of continuous metal, and of imperfectly conducting material, such as paper, slate, wood, and a sheet of vulcanite, moist or dry, window glass at ordinary temperatures in air of ordinary moisture, and by perforated metal screens and screens of network, or gratings of parallel bars.

The fixing of an electric image is shown in two experiments: (1) the image of a stick of sealing-wax in a thin plane sheet of vulcanite, moistened, warmed, and dried under the electric influence by the application and removal of a spirit-lamp flame; (2) the glass jar of a quadrant electrometer with a rubbed stick of sealing-wax held projecting into it, while the outer surface is moistened, warmed, and dried by the application and removal of a ring of flame produced by cotton wick wrapped on an iron ring and moistened in alcohol.

Fig. 5 is copied from a diagram of Clerk Maxwell's to illustrate screening by a plane grating of parallel bars of approximately circular

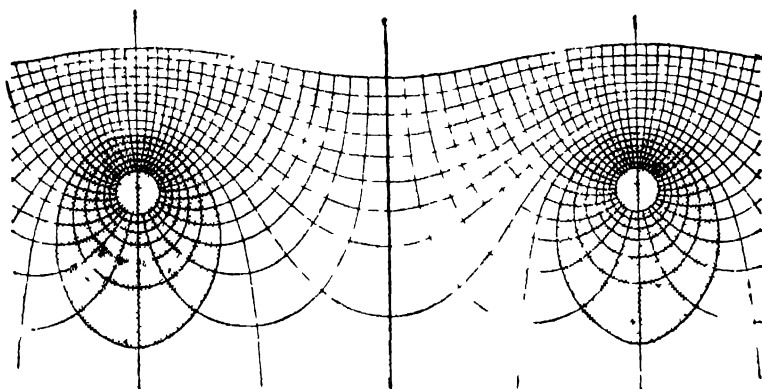


FIG. 5.

cross section, with distance from centre to centre twelve times the diameter of each bar.\* It represents the lines of force due to equal quantities of opposite electricities on the grating itself, and a parallel plane of continuous metal (not shown in the diagram) at a distance from the grating of not less than one and a half times the distance from bar to bar. The shading shows the lines of force for the same circumstances, but with oval bars instead of the small circular bars of Maxwell's grating. It is interesting to see how every line of force ends in a bar of the grating, none straying to an infinite distance beyond it, which is necessarily the case when the quantities of electricity on the grating and on the continuous plane are equal and opposite. If an insulated electrified body, with electricity of the same name as that of the grating, for example, is brought up from below, it experiences no electric force differing sensibly from that which would be produced by its own inductive effect on the grating, till it is within a less distance from the grating than the distance from bar to bar, when it experiences repulsion or attraction, according as it is under a bar of the grating or under the middle of a space between two bars. If there be a parallel metal plane below the grating, kept

\* 'Electricity and Magnetism,' vol. i. art. 203, fig. xiii.

at the same potential as the grating, it takes no sensible proportion of the electricity from the grating, and experiences no sensible force when its distance from the grating exceeds a limit depending on the ratio of the diameter of each bar to the distance from bar to bar. The mathematical theory of this action was partially given by Maxwell,\* and yesterday I communicated an extension of it to the Royal Society.

## II.

Magnetostatic screening by soft iron would follow the same law as electrostatic screening, if the magnetic susceptibility of the iron were infinitely great. It is not great enough to even approximately

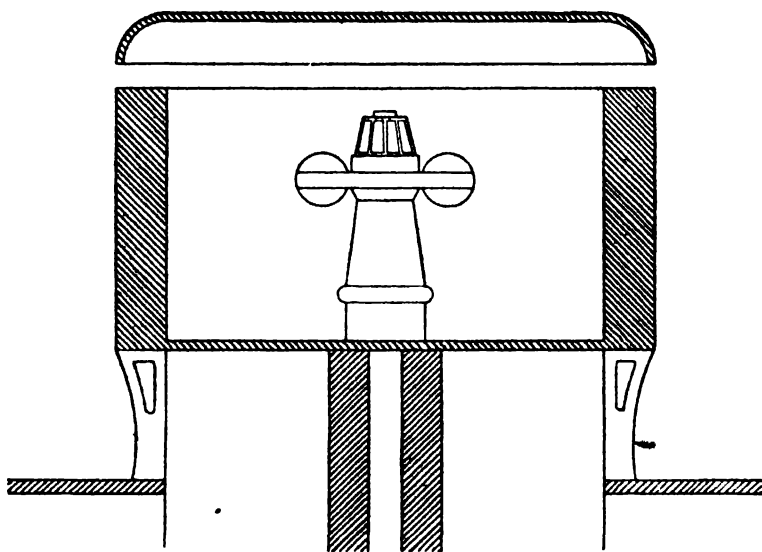


FIG. 6.

fulfil this condition in any practical case. The nearest approach to fulfilment is presented when we have a thick iron shell completely enclosing a hollow space, but the thickness must be a considerable proportion of the smallest diameter, not less than  $\frac{1}{10}$ , perhaps, for iron of ordinary magnetic susceptibility to produce so much of screening effect that the magnetic force in the interior should be anything less than 5 per cent. of the force at a distance outside, when the shell is placed in a uniform magnetic field. The accompanying diagram, Fig. 6, representing the conning-tower of H.M.S. '*Orlando*,' and the position of the compass within it, has been kindly sent to me by Captain Creak, R.N., for this lecture, by permission of the Controller

\* Arts. 203-205.



of the Navy. It gives an interesting illustration of magnetic screening effect by the case of a belt of iron, 1 foot thick, 5 feet high, and 10 feet in internal diameter, with roof and floor of comparatively thin iron. Captain Creak informs me that the average horizontal component of the magnetic directing force on the compass in the centre of this conning-tower is only about one-fifth of that of the undisturbed terrestrial magnetism.

An evil practice, against which careful theoretical and practical warnings were published two or three years ago,\* and which is now nearly, though, I believe, not at this moment quite thoroughly, stopped, of what is called single wiring in the electric lighting of ships, has been fallaciously defended by various bad reasons, among them an erroneous argument that the ship's iron produced a sufficient screening effect against disturbance of the ship's compasses, by the electric light currents, when that plan of wiring is adopted. The argument would be good for a ship 50 feet broad and 30 feet deep, if the deck and hull were of iron 3 feet thick. As it is, mathematical calculation shows that the screening effect is quite small in comparison with what the disturbance of the compass would be if the ship and her decks were all of wood. Actual observation, on ships electrically lighted on the single wire system by some of the best electrical engineers in the world, has shown, in many cases, disturbance of the compass of from 3 degrees to 7 degrees, produced by throwing off and on the groups of lights in various parts of the ship, which are thrown on and off habitually in the evenings and nights, in ordinary and necessary practice of sea-going passenger ships. When the facts become known to shipowners, single wiring will never again be admitted at sea unless the alternating current system of electric lighting is again adopted. But, although this system was largely used when electric lighting was first introduced into ships, the economy and other advantages of the direct-current system are so great that no one would think of using the alternate system for the trivial economy, *if any economy there is*, in the single wire, as compared with the double insulated wire system.

An interesting illustration of a case in which iron, of any thickness, however great, produces *no screening effect* on an electric current, steady or alternating, is shown by the accompanying diagram, Fig. 7, which represents in section an electric current along the axis of a circular iron tube, completely surrounding it. Whether the tube be long or short, it exercises no screening effect whatever. A single circular iron ring, supported in the air, with its plane perpendicular to the length of a straight conductor conveying an electric current, produces absolutely no disturbance of the circular endless lines of magnetic force which surround the wire; neither does any piece of

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\* See 'The Electrician,' vol. xxiii. p. 87. Paper read before the Institution of Electrical Engineers, by Sir William Thomson, "On the Security against Disturbance of Ships' Compasses by Electric Lighting Appliances."

iron, wholly bounded by a surface of revolution, with a straight conductor conveying electricity along its axis.

A screen of imperfectly conducting material is as thorough in its action, when time enough is allowed it, as is a similar screen of metal. But if it be tried against rapidly varying electrostatic force,

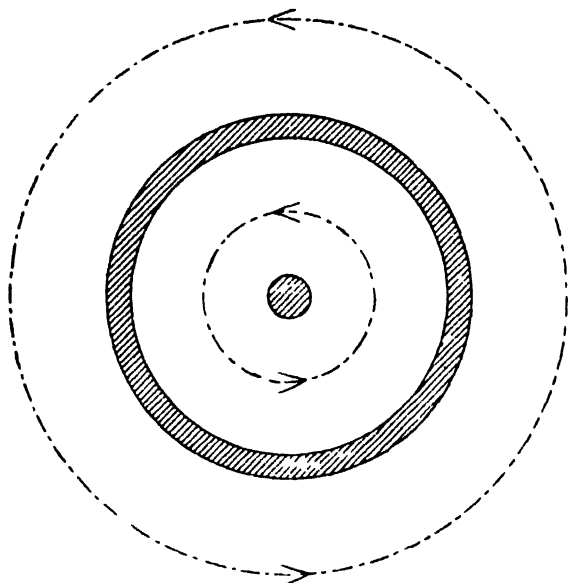


FIG. 7.

its action lags. On account of this lagging, it is easily seen that the screening effect against periodic variations of electrostatic force will be less and less, the greater the frequency of the variation. This is readily illustrated by means of various forms of idiostatic electrometers. Thus, for example, a piece of paper supported on metal in metallic communication with the movable disc of an attracted disc electrometer annuls the attraction (or renders it quite insensible) a few seconds of time after a difference of potential is established and kept constant between the attracted disc and the opposed metal plate, if the paper and the air surrounding it are in the ordinary hygrometric conditions of our climate. But if the instrument is applied to measure a rapidly alternating difference of potential, with equal differences on the two sides of zero, it gives very little less than the same average force as that found when the paper is removed and all other circumstances kept the same. Probably, with ordinary clean white paper in ordinary hygrometric conditions, a frequency of alternation of from 50 to 100 per second will more than suffice to render the screening influence of the paper insensible. And a much

less frequency will suffice if the atmosphere surrounding the paper is artificially dried. Up to a frequency of millions per second, we may safely say that, the greater the frequency, the more perfect is the annulment of screening by the paper; and this statement holds also if the paper be thoroughly blackened on both sides with ink, although possibly in this condition a greater frequency than 50 to 100 per second might be required for practical annulment of the screening.

Now, suppose, instead of attractive force between the two bodies separated by the screen, as our test of electrification, that we have as test a faint spark, after the manner of Hertz. Let two well insulated metal balls, A, B, be placed very nearly in contact, and two much larger balls, E, F, placed beside them, with the shortest distance between E, F sufficient to prevent sparking, and with the lines joining the centres of the two pairs parallel. Let a rapidly alternating difference of potential be produced between E and F, varying, not abruptly, but according, we may suppose, to the simple harmonic law. Two sparks in every period will be observed between A and B. The interposition of a large paper screen between E, F, on one side, and A, B, on the other, in ordinary hygrometric conditions, will absolutely stop these sparks, if the frequency be less than, perhaps, 4 or 5 per second. With a frequency of 50 or more, a clean white paper screen will make no perceptible difference. If the paper be thoroughly blackened with ink on both sides, a frequency of something more than 50 per second may be necessary; but some moderate frequency of a few hundreds per second will, no doubt, suffice to practically annul the effect of the interposition of the screen. With frequencies up to 1000 million per second, as in some of Hertz's experiments, screens such as our blackened paper are still perfectly transparent, but if we raise the frequency to 500 million million, the influence to be transmitted is light, and the blackened paper becomes an almost perfect screen.

Screening against a varying magnetic force follows an opposite law to screening against varying electrostatic force. For the present I pass over the case of iron and other bodies possessing magnetic susceptibility, and consider only materials devoid of magnetic susceptibility, but possessing more or less of electric conductivity. However perfect the electric conductivity of the screen may be, it has no screening efficiency against a steady magnetic force. But if the magnetic force varies, currents are induced in the material of the screen which tend to diminish the magnetic force in the air on the remote side from the varying magnet. For simplicity, we shall suppose the variations to follow the simple harmonic law. The greater the electric conductivity of the material, the greater is the screening effect for the same frequency of alternation; and, the greater the frequency, the greater is the screening effect for the same material. If the screen be of copper, of specific resistance 1640 sq. cm. per second (or electric diffusivity 130 sq. cm. per second), and with frequency 80 per second, what I have called the "mhoic effective

thickness" is 0.71 of a cm.; and the range of current intensity at depth  $n \times 0.71$  cm. from the surface of the screen next the exciting magnet is  $e^{-n}$  of its value at the surface.

Thus (as  $e^3 = 20.09$ ) the range of current intensity at depth 2.13 cm. is  $\frac{1}{10}$  of its surface value. Hence we may expect that a sufficiently large plate of copper of  $2\frac{1}{2}$  cm. thick will be a little less than perfect in its screening action against an alternating magnetic force of frequency 80 per second.

Lord Rayleigh, in his "Acoustical Observations,"† after referring to Maxwell's statement, that a perfectly conducting sheet acts as a barrier to magnetic force,‡ describes an experiment in which the interposition of a large and stout plate of copper between two coils renders inaudible a sound which, without the copper screen, is heard by a telephone in circuit with one of the coils excited by electromagnetic induction from the other coil, in which an intermittent current, with sudden, sharp variations of strength, is produced by a "microphone clock" and a voltaic battery. Larmor, in his paper on "Electromagnetic Induction in Conducting Sheets and Solid Bodies"§ makes the following very interesting statement:—"If we have a sheet of conducting matter in the neighbourhood of a magnetic system, the effect of a disturbance of that system will be to induce currents in the sheet of such kind as will tend to prevent any change in the conformation of the tubes [lines] of force cutting through the sheet. This follows from Lenz's law, which itself has been shown by Helmholtz and Thomson to be a direct consequence of the conservation of energy. But if the arrangement of the tubes [lines of force] in the conductor is unaltered, the field on the other side of the conductor into which they pass (supposed isolated from the outside spaces by the conductor) will be unaltered. Hence, if the disturbance is of an alternating character, with a period small enough to make it go through a cycle of changes before the currents decay sensibly, we shall have the conductor acting as a screen.

"Further, we shall also find, on the same principle, that a rapidly rotating conducting sheet screens the space inside it from all magnetic action which is not symmetrical round the axis of rotation."

Mr. Willoughby Smith's experiments on "Volta-electric induction," which he described in his inaugural address to the Society of Telegraph Engineers of November 1883, afforded good illustration of this kind of action with copper, zinc, tin, and lead, screens, and with different degrees of frequency of alternation. His results with iron are also very interesting: they showed, as might be expected, comparatively little augmentation of screening effect with augmentation of frequency. This is just what is to be expected from the fact

\* 'Collected Papers,' vol. 3, art. cii. § 35.

† Phil. Mag. 1882, first half-year.

‡ 'Electricity and Magnetism,' § 665.

§ Phil. Mag. 1884, first half-year.

that a broad enough and long enough iron plate exercises a large magneto-static screening influence; which with a thick enough plate, will be so nearly complete that comparatively little is left for augmentation of the screening influence by alternations of greater and greater frequency.

A copper shell closed around an alternating magnet produces a screening effect which on the principle stated above we may reckon to be little short of perfection if the thickness be  $2\frac{1}{4}$  cm. or more, and the frequency of alternation 80 per second.

Suppose now the alternation of the magnetic force to be produced by the rotation of a magnet *M* about any axis. First, to find the effect of the rotation, imagine the magnet to be represented by ideal magnetic matter. Let (after the manner of Gauss in his treatment of the secular perturbations of the solar system) the ideal magnetic matter be uniformly distributed over the circles described by its different points. For brevity call *I* the ideal magnet symmetrical round the axis, which is thus constituted. The magnetic force throughout the space around the rotating magnet will be the same as that due to *I*, compounded with an alternating force of which the component at any point in the direction of any fixed line varies from zero in the two opposite directions in each period of the rotation. If the copper shell is thick enough, and the angular velocity of the rotation great enough, the alternating component is almost annulled for external space, and only the steady force due to *I* is allowed to act in the space outside the copper shell.

Consider now, in the space outside the copper shell, a point *P* rotating with the magnet *M*. It will experience a force simply equal to that due to *M* when there is no rotation, and, when *M* and *P* rotate together, *P* will experience a force gradually altering as the speed of rotation increases, until, when the speed becomes sufficiently great, it becomes sensibly the same as the force due to the symmetrical magnet *I*. Now superimpose upon the whole system of the magnet, and the point *P*, and the copper shell, a rotation equal and opposite to that of *M* and *P*. The statement just made with reference to the magnetic force at *P* remains unaltered, and we have now a fixed magnet *M* and a point *P* at rest, with reference to it, while the copper shell rotates round the axis around which we first supposed *M* to rotate.

A little piece of apparatus, constructed to illustrate the result experimentally, was submitted to the Royal Institution and shown in action. The copper shell is a cylindric drum, 1.25 cm. thick, closed at its two ends with circular discs 1 cm. thick. The magnet is supported on the inner end of a stiff wire passing through the centre of a perforated fixed shaft which passes through a hole in one end of the drum, and serves as one of the bearings; the other bearing is a rotating pivot fixed to the outside of the other end of the drum. The accompanying sections, drawn to a scale of three-fourths full size, explain the arrangement sufficiently. A magnetic needle outside,

deflected by the fixed magnet when the drum is at rest, shows a great diminution of the deflection when the drum is set to rotate. If the

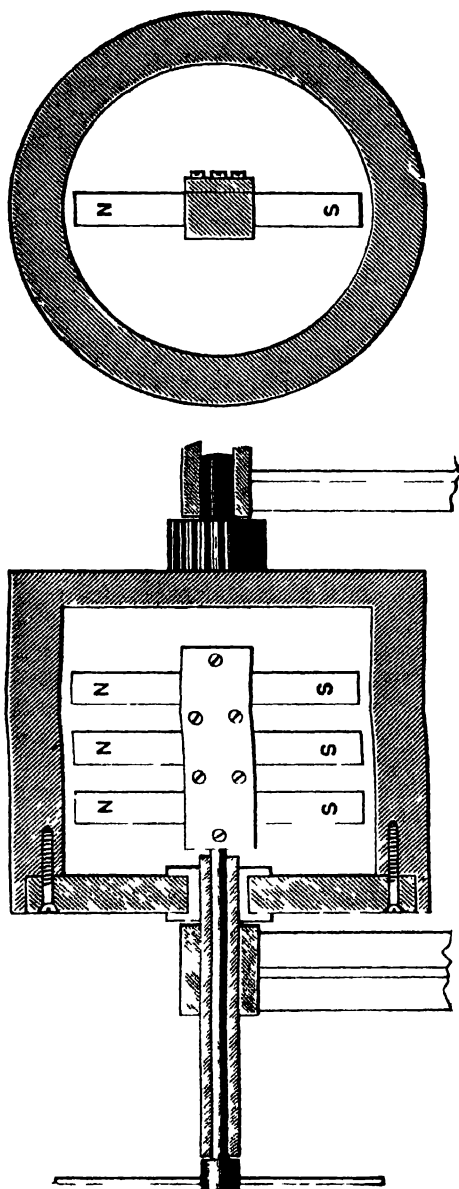


FIG. 8.

(triple compound) magnet inside is reversed, by means of the central wire and cross bar outside, shown in the diagram, the magnetometer outside is greatly affected while the copper shell is at rest; but scarcely affected perceptibly while the copper shell is rotating rapidly.

When the copper shell is a figure of revolution, the magnetic force at any point of the space outside or inside is steady, whatever be the speed of rotation; but if the shell be not a figure of revolution, the steady force in the external space observable when the shell is at rest becomes the resultant of the force due to a fixed magnet intermediate between  $M$  and  $I$  compounded with an alternating force with amplitude of alternation increasing to a maximum, and ultimately diminishing to zero, as the angular velocity is increased without limit.

If  $M$  be symmetrical, with reference to its northern and southern polarity, on the two sides of a plane through the axis of rotation,  $I$  becomes a null magnet, the ideal magnetic matter in every circle of which it is constituted being annulled by equal quantities of positive and negative magnetic matter being laid on it. Thus, when the rotation is sufficiently rapid, the magnetic force is annulled throughout the space external to the shell. The transition from the steady force of  $M$  to the final annulment of force, when the copper shell is symmetrical round its axis of rotation, is, through a steadily diminishing force, without alternations. When the shell is not symmetrical round its axis of rotation, the transition to zero is accompanied with alternations as described above.

When  $M$  is not symmetrical on the two sides of a plane through the axis of rotation,  $I$  is not null; and the condition approximated to through external space with increasing speed of rotation is the force due to  $I$ , which is an ideal magnet symmetrical round the axis of rotation.

A very interesting simple experimental illustration of screening against magnetic force may be shown by a rotating disc with a fixed magnet held close to it on one side. A bar magnet held with its magnetic axis bisected perpendicularly by a plane through the axis of rotation would, by sufficiently rapid rotation, have its magnetic force almost perfectly annulled at points in the air as near as may be to it, on the other side of the disc, if the diameter of the disc exceeds considerably the length of the magnet. The magnetic force in the air close to the disc, on the side next to the magnet, will be everywhere parallel to the surface of the disc.

[W. T.]

Friday, May 8, 1891.

SIR FREDERICK BRAMWELL, Bart. D.C.L. F.R.S. Honorary Secretary  
and Vice-President, in the Chair.

PROFESSOR W. RAMSAY, Ph.D. F.R.S. M.R.I.

*Liquids and Gases.*

ALMOST exactly twenty years ago, on June 2nd, 1871, Dr. Andrews, of Belfast, delivered a lecture to the Members of the Royal Institution in this Hall, on "The Continuity of the Gaseous and the Liquid states of Matter." He showed in that lecture an experiment which I had best describe in his own words:—

"Take, for example, a given volume of carbonic acid at 50° Centigrade, or at a higher temperature, and expose it to increasing pressure till 150 atmospheres have been reached. In the process its volume will steadily diminish as the pressure augments; and no sudden diminution of volume, without the application of external pressure, will occur at any stage of it. When the full pressure has been applied, let the temperature be allowed to fall until the carbonic acid has reached the ordinary temperature of the atmosphere. During the whole of this operation no break of continuity has occurred. It begins with a gas, and by a series of gradual changes, presenting nowhere any abrupt alteration of volume or sudden evolution of heat, it ends with a liquid.

"For convenience, the process has been divided into two stages, the compression of the carbonic acid and its subsequent cooling. But these operations might have been performed simultaneously if care were taken so to arrange the application of the pressure and the rate of cooling that the pressure should not be less than seventy-six atmospheres when the carbonic acid has cooled to 31°."

I am able, through the kindness of Dr. Letts, Dr. Andrews' successor at Belfast, to show you this experiment, with the identical piece of apparatus used on the occasion of the lecture twenty years ago.

I must ask you to spend some time to-night in considering this remarkable behaviour; and in order to obtain a correct idea of what occurs, it is well to begin with a study of gases, not, as in the case you have just seen, exposed to high pressures, but under pressures not differing greatly from that of the atmosphere, and at temperatures which can be exactly regulated and measured. To many here to-night such a study is unnecessary, owing to its familiarity, but I will ask such of my audience to excuse me, in order that I may tell my story from the beginning.



Generally speaking, a gas, when compressed, decreases in volume to an amount equal to that by which its pressure is raised, provided its temperature be kept constant. This was discovered by Robert Boyle in 1660. (In 1661 he presented to the Royal Society a Latin translation of his book 'Touching the Spring of the Air and its Effects.') His words are:—

"'Tis evident that as common air, when reduced to half its natural extent, obtained a spring about twice as forcible as it had before; so the air, being thus compressed, being further crowded into half this narrow room, obtained a spring as strong again as that it last had, and consequently four times as strong again as that of common air."

To illustrate this, and to show how such relations may be expressed by a curve, I will ask your attention to this model. We have a piston, fitting a long glass tube. It confines air under the pressure of the atmosphere, that is, some 15 lb. on each square inch of area of the piston. The pressure is supposed to be registered by the height of the liquid in the vertical tube. On increasing the volume of the air, so as to double it, the pressure is decreased to half its original amount. On decreasing the volume to half its original amount the pressure is doubled. On again halving, the pressure is again doubled. Thus, you see, a curve may be traced, in which the relation of volume to pressure is exhibited. Such a curve, it may be remarked incidentally, is termed a hyperbola.

We can repeat Boyle's experiment by pouring mercury into the open limb of this tube containing a measured amount of air. On causing the level of the mercury in the open limb to stand 30 inches (that is the height of the barometer) higher in the open limb than the closed limb, the pressure of the atmosphere is doubled, and the volume is halved. And on trebling the pressure of the atmosphere the volume is reduced to one-third of its original amount, and on adding other 30 inches of mercury the volume of the air is now one quarter of that which it originally occupied.

It must be remembered that here the temperature is kept constant; that it is the temperature of the surrounding atmosphere.

Let us next examine the behaviour of a gas when its temperature is altered; when it becomes hotter. This tube contains a gas, air, confined by mercury, in a tube surrounded by a jacket or mantle of glass, and the vapour of boiling water can be blown into the space between the mantle and the tube containing the air, so as to heat the tube to 100°, the temperature of the steam. The temperature of the room is 17° C., and the gas occupies 290 divisions of the scale. On blowing in steam the gas expands, and on again equalising pressure it stands at 373 divisions of the scale. The gas has thus expanded from 290 to 373 divisions, i. e. its volume has increased by 83 divisions, and the temperature has risen from 17° to 100°, i. e. through 83 degrees. This law of the expansion of gases was discovered almost simultaneously by Dalton and Gay-Lussac in 1801. It usually

goes by the name of Gay-Lussac's law. Now, if we do not allow the volume of the gas to increase, we shall find that the pressure will increase in the same proportion that the volume would have increased had the gas been allowed to expand, the pressure having been kept constant. To decrease the volume of the gas, then, according to Boyle's law, will require a higher initial pressure, and if we were to represent the results by a curve we should get a hyperbola, as before, but one lying higher as regards pressures. And so we should get a set of hyperbolas for higher and higher temperatures.

We have experimented up to the present with air, a mixture of two gases, oxygen and nitrogen; and the boiling-point of both of these elements lies at very low temperatures,  $-184^{\circ}$  and  $-193.1^{\circ}$  respectively. The ordinary atmospheric temperature lies a long way above the boiling-points of liquid oxygen and liquid nitrogen at the ordinary atmospheric pressure. But it is open to us to study a gas, which, at the ordinary atmospheric temperature exists in the liquid state; and for this purpose I shall choose water-gas; in order that it may be a gas at ordinary atmospheric pressure, however, we must heat it to a temperature above  $100^{\circ}$  C., its boiling-point. This tube contains water-gas at a temperature of  $105^{\circ}$  C.; it is under ordinary pressure, for the mercury columns are at the same level in both the tube and in this reservoir, which communicates with the lower end of the tube by means of the india-rubber tubing. The temperature,  $105^{\circ}$ , is maintained by the vapour of chlorobenzene, boiling in the bulb sealed to the jacket, at a pressure lower than that of the atmosphere.

Let us now examine the effect of increasing pressure. On raising the reservoir, the volume of the gas is diminished, as usual, and nearly in the ratio given by Boyle's law; that is, the volume decreases in the same proportion as the pressure increases. But a change is soon observed; the pressure soon ceases to rise; the distance between the mercury in the reservoir and that in the tube remains constant, and the gas is now condensing to liquid. The pressure continues constant during this change, and it is only when all the water-gas has condensed to liquid water that the pressure again rises. After all gas is condensed, an enormous increase of pressure is necessary to cause any measurable decrease in volume, for liquid water scarcely yields to pressure, and in such a tube as this no measurements could be attempted with success.

Representing this diagrammatically, the right-hand part of the curve represents the compression of the gas, and the curve is, as before, nearly a hyperbola. Then comes a break, and great increase in volume occurs without rise of pressure, represented by a horizontal line. The substance in the tube here consists of water-gas in presence of water; the vertical, or nearly vertical, line represents the sudden and great rise of pressure, where liquid water is being slightly compressed. The pressure registered by the horizontal line

is termed the "vapour-pressure" of water. If now the temperature were raised to  $110^{\circ}$  we should have a greater initial volume for the water-gas. It is compressible, by rise of the mercury, as before, the relation of pressure to volume being, as before, represented on the

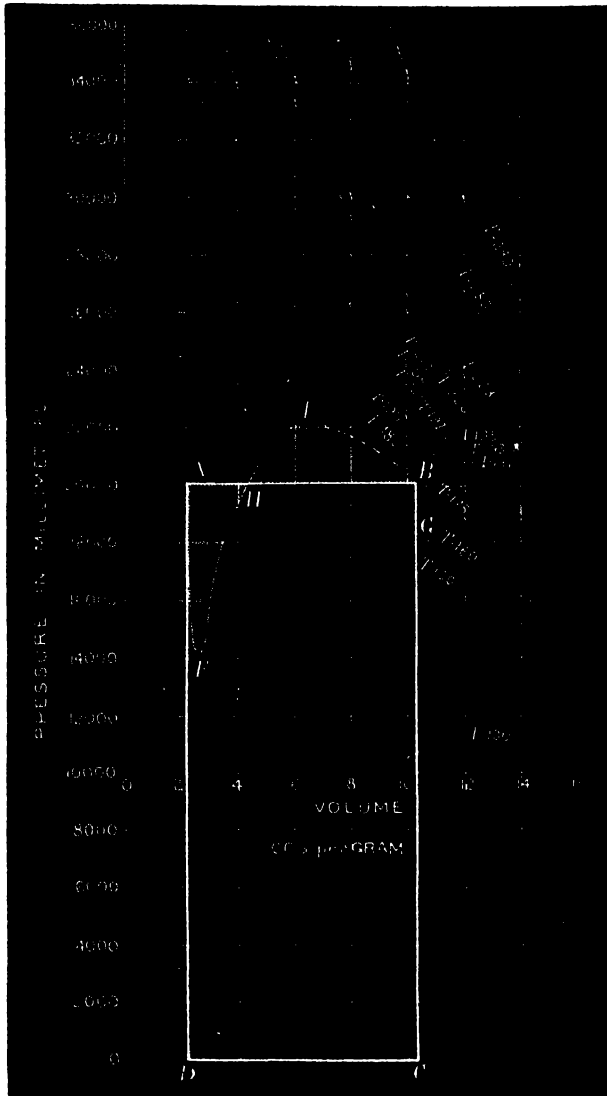


diagram as an approximate hyperbola; and, as before, condensation occurs when volume is sufficiently reduced but this time at a higher pressure. We have again a horizontal portion, representing the pressure of water-gas at  $110^{\circ}$  in contact with liquid water; again a

sharp angle, where all gaseous water is condensed ; and again a very steep curve, almost a straight line, representing the slight decrease of volume of water, produced by a great increase of pressure. And should we have similar lines for  $120^{\circ}$ ,  $130^{\circ}$ ,  $140^{\circ}$ ,  $150^{\circ}$ , and for all temperatures—such lines are called isothermal lines, or shortly, “isothermals,” or lines of equal temperature, and represent the relations of pressure to volume for different temperatures. Dr. Andrews made similar measurements of the relations between the pressure and volumes of carbon dioxide, at pressures much higher than those I have shown you for water. But I prefer to speak to you about similar results obtained by Professor Sydney Young and myself with ether, because Dr. Andrews was unable to work with carbon dioxide free from air, and that influenced his results. For example, you see that the meeting-points of his hyperbolic curves with the straight lines of vapour-pressures are curves and not angles. That is caused by the presence of about 1 part of air in 500 parts of carbon dioxide ; also, the condensation of gas was not perfect, for he obtained curves at the points of change from a mixture of liquid and gas to liquid. We, however, were more easily able to fill a tube with ether, free from air, and you will notice that the points I have referred to are angles, not curves.

Let me first direct your attention to the shapes of the curves in the figure, which represents such relations of volume, temperature, and pressure in the case of ether. As the temperature rises, the vapour-pressure lines lie at higher and higher pressures, and the lines themselves become shorter and shorter. And finally, at the temperature  $31^{\circ}$  for carbon dioxide, and at  $125^{\circ}$  for ether, there ceases to be a horizontal portion at all ; or, rather, the curve touches the horizontal at one point in its course. That point corresponds to a definite temperature,  $195^{\circ}$  for ether ; to a definite pressure, 27 metres of mercury, or  $35\cdot6$  atmospheres ; and to a definite volume,  $4\cdot06$  cubic centimetres per gram of ether. At that point the ether is not liquid, and it is not gas ; it is a homogeneous substance. At that temperature ether has the appearance of a blue mist. The stripe mentioned by Dr. Andrews and by other observers are the result of unequal heating, one portion of the substance being liquid and another gas. You see the appearance of this state on the screen.

When a gas is compressed, it is heated. Work is done on the gas, and its temperature rises. If I compress the air in this syringe forcibly, its temperature rises so high that I can set a piece of tinder on fire, and by its help explode a little gunpowder. If the ether at its critical point be compressed, by screwing in the screw, it is somewhat warmed, and the blue cloud disappears. Conversely, if it is expanded a little by unscrewing the screw, and increasing its volume, it is cooled, and a dense mist is seen accompanied by a shower of ether rain. This is seen as a black fog on the screen.

I wish also to direct your attention to what happens if the volume

given to the ether is greater than the critical volume. On increasing the volume, you see that it boils away, and evaporates completely; and also what happens if the volume be somewhat less than the critical volume; it then expands as liquid, and completely fills the tube. It is only at a critical volume and temperature that the ether exists in the state of blue cloud, and has its critical pressure. If the volume be too great, the pressure is below the critical pressure; if too small, the pressure is higher than the critical pressure.

Still one more point before we dismiss this experiment. At a temperature some degrees below the critical temperature, the meniscus, i. e. the surface of the liquid is curved. It has a skin on its surface; its molecules, as Lord Rayleigh has recently explained in this room, attract one another, and it exhibits surface tension. Raise the temperature, and the meniscus grows flatter; raise it further, and it is nearly flat, and almost invisible; at the critical temperature it disappears, having first become quite flat. Surface-tension therefore disappears at the critical point. A liquid would no longer rise in a narrow capillary tube; it would stand at the same level outside and inside.

It was suggested by Professor James Thomson and by Professor Clausius, about the same time, that if the ideal state of things were to exist, the passage from the liquid to the gaseous state should be a continuous one, not merely at and above the critical point, but below that temperature. And it was suggested that the curves shown in the figure, instead of breaking into the straight line of vapour-pressure, should continue sinuously. Let us see what this conception would involve.

On decreasing the volume of a gas, it should not liquefy at the point marked B on the diagram, but should still decrease in volume on increase of pressure. This decrease should continue until the point E is reached. The anomalous state of matters should then occur, that a decrease in volume should be accompanied by a decrease of pressure. In order to lessen volume, the gas must be exposed to a continually diminishing pressure. But such a condition of matter is of its nature unstable, and has never been realised. After volume has been decreased to a certain point F, decrease of volume is again attended by increase of pressure, and the last part of the curve is continuous with the realisable curve representing the compression of the liquid above D.

Dr. Sydney Young and I succeeded by a method which I shall briefly describe in calculating the actual position of the unrealisable portions of the curve. They have the form pictured in the figure (shaded portion). The rise from the gaseous state is a gradual one; but the fall from the liquid state is abrupt.

Consider the volume 14 cubic centimetres per gram on the figure. The vertical equivolume line cuts the isothermal lines for the

temperatures  $175^{\circ}$ ,  $180^{\circ}$ ,  $185^{\circ}$ ,  $190^{\circ}$ , and so on, at certain definite pressures, which may be read from a properly constructed diagram. We can map the course of lines of equal volume, of which the instance given is one, using temperatures as ordinates and pressures as abscissæ. We can thus find the relations of temperature to pressure for certain definite volumes, which we may select to suit our convenience; say, 2 c.c. per gram, 3, 4, 5, 6, and so on. Now all such lines are straight. That is, the relation of pressure to temperature, at constant volume, is one of the simplest; pressure is a linear function of temperature measured on the absolute scale. Expressed mathematically,

$$p = bt - a,$$

where  $b$  and  $a$  are constants, depending on the volume chosen, and varying with each volume. But a straight line may be extrapolated without error, and so having found values for  $a$  and  $b$  for such a volume as 6 c.c. per gram, by help of experiments at temperatures higher than  $195^{\circ}$ , it is possible by extrapolation to obtain the pressures corresponding to temperatures below the critical point  $195^{\circ}$ , in a simple manner. But below that temperature the substance at volume 6 is in practice partly liquid and partly gas. Yet it is possible by such means to ascertain the relations of pressure to temperature for the *unrealisable portion* of the state of a liquid, that is, we can deduce the pressure and temperature corresponding to a continuous change from liquid to gas. And in this manner the sinuous lines on the figure have been constructed.

It is possible to realise experimentally certain portions of such continuous curves. If we condense all gaseous ether, and, when the tube is completely filled with liquid, carefully reduce pressure, the pressure may be lowered considerably below the vapour pressure corresponding to the temperature of ebullition without any change, further than the slight expansion of the liquid resulting from the reduction of pressure—an expansion too small to be seen with this apparatus. But on still further reducing pressure sudden ebullition occurs, and a portion of the liquid suddenly changes into gas, while the pressure rises quickly to the vapour-pressure corresponding to the temperature. If we are successful in expelling all air or gas from the ether in filling the tube, a considerable portion of this curve can be experimentally realised.

The first notice of this appearance, or rather, of one owing its existence to a precisely similar cause, is due to Mr. Hooke, the celebrated contemporary of Boyle. It is noted in the account of the Proceedings of the Royal Society, on November 6th, 1672, that "Mr. Hooke read a discourse of his, containing his thoughts of the experiment of the quicksilver's standing top-full, and far above the 29 inches; together with some experiments made by him, in order to determine the cause of this strange phenomenon. He was ordered to prepare those experiments for the view of the Society." And on

November 13th, "The experiment for the high suspension of quick-silver being called for, it was found that it had failed. It was ordered that thicker glasses should be provided for the next meeting."

There can be no doubt that this behaviour is caused by the attraction of the molecules of the liquid for each other. And if the temperature be sufficiently low, the pressure may be so reduced that it becomes negative—that is, until the liquid is exposed to a strain or pull, as is the mercury. This has been experimentally realised by M. Berthelot, and by Mr. Worthington, the latter of whom has succeeded in straining alcohol at the ordinary temperature with a pull equivalent to a negative pressure of 25 atmospheres, by completely filling a bulb with alcohol and then cooling it. The alcohol in contracting strains the bulb inwards, and finally, when the tension becomes very great, parts from the glass with a sharp "click."

To realise a portion of the other bend of the curve, an experiment has been devised by Mr. John Aitken. It is as follows: If air (that is space, for the air plays a secondary part) saturated with moisture be cooled, the moisture will not deposit unless there are dust-particles on which condensation can take place. It is not at first evident how this corresponds to the compressing of a gas without condensation. But a glance at the figure will render the matter plain. Consider the isothermal (175') 75' for ether at the point marked B. If it were possible to lower the temperature to 160° without condensation, keeping volume constant, pressure would fall, and the gas would then be in the state represented on the isothermal line 160° at G; that is, it would be in the same condition as if it had been compressed without condensation.

You saw that a gas, or a liquid, is heated by compression; a piece of tinder was set on fire by the heat evolved on compressing air. You saw that condensation of ether was brought about by diminution of pressure; that is, it was cooled. Now if air be suddenly expanded it will do work against atmospheric pressure, and will cool itself. This globe contains air; but the air has been filtered carefully through cotton-wool, with the object of excluding dust-particles. It is saturated with moisture. On taking a stroke of the pump, so as to exhaust the air in the globe, no change is evident; no condensation has occurred, although the air has been so cooled that the moisture should condense, were it possible. On repeating the operation with the same globe after admitting dusty air—ordinary air from the room—a slight fog is produced, and owing to the light behind, a circular rainbow is seen; a slight shower of rain has taken place. There are comparatively few dust-particles, because only a little dusty air has been admitted. On again repeating, the fog is denser; there are more particles on which moisture may condense.

One point more and I have done. Work is measured by the distance or height through which a weight can be raised against the force of gravity. The British unit of work is a foot-pound, that is, a pound raised through one foot; that of the metric system is one gramme

raised through one centimetre. If a pound be raised through two feet, twice as much work is done as that of raising a pound through one foot, and an amount equal to that of raising two pounds through one foot. The measure of work is then the weight, multiplied by the distance through which it is raised. When a gas expands against pressure it does work. The gas may be supposed to be confined in a vertical tube, and to propel a piston upwards against the pressure of the atmosphere. If such a tube has a sectional area of one square centimetre, the gas in expanding a centimetre up the tube lifts a weight of nearly 1000 grams through one centimetre (for the pressure of the atmosphere on a square centimetre of surface is nearly 1000 grams); that is, it does 1000 units of work, or ergs. So the work done by a gas in expanding is measured by the change of volume multiplied by the pressure. On the figure the change of volume is measured horizontally, the change of pressure vertically. Hence the work done is equivalent to the area on the diagram A B C D.

If liquid as it exists at A change to gas as it exists at B, the substance changes its volume, and may be made to do work. This is familiar in the steam-engine, where work is done by water expanding to steam, and so increasing its volume. The pressure does not alter during this change of volume if sufficient heat be supplied, hence the work done during such a change is given by the rectangular area.

Suppose that a man is conveying a trunk up to the first storey of a house, he may do it in two (or perhaps a greater number of) ways. He may put a ladder up to the drawing-room window, shoulder his trunk, and deposit it directly on the first floor. Or he may go down the area stairs, pass through the kitchen, up the kitchen stairs, up the first flight, up the second flight, and down again to the first storey. The end result is the same; and he does the same amount of work in both cases, so far as conveying the weight to a given height is concerned; because in going downstairs he has actually allowed work to be done on him by the descent of the weight.

Now the liquid in expanding to gas begins at a definite volume; it evaporates gradually to gas without altering pressure, heat being of course communicated to it during the change, else it would cool itself; and it finally ends as gas. It increases its volume by a definite amount at a definite pressure, and so does a definite amount of work; this work might be utilised in driving an engine.

But if it pass continuously from liquid to gas, the starting-point and the end point are both the same as before. An equal amount of work has been done. But it has been done by going down the area-stair, as it were, and over the round I described before.

It is clear that a less amount of work has been done on the left-hand side of the figure than was done before; and a greater amount on the right-hand side; and if I have made my meaning clear, you will see that as much less has been done on the one side, as more has been done on the other; that is, that the area of the figure B E H



must be equal to that of the figure A F H. Dr. Young and I have tried this experimentally, that is, by measuring the calculated areas; and we found them to be equal. This can be shown to you easily by a simple device, namely, taking them out and weighing them. As this diagram is an exact representation of the results of our experiments with ether, the device can be put in practice. We can detach these areas which are cut out in tin, and place one in each of this pair of scales, and they balance. The fact that a number of areas thus measured gave the theoretical results, of itself furnishes a strong support of the justice of the conclusions we drew as regards the forms of these curves.

To attempt to explain the reasons of this behaviour would take more time than can be given to-night; moreover, to tell the truth, we do not know them. But we have at least partial knowledge; and we may hope that investigations at present being carried out by Professor Tait may give us a clear idea of the nature of the matter, and of the forces which act on it, and with which it acts, during the continuous change from gas to liquid.

[W. R.]

Friday, May 15, 1891.

SIR JAMES CRICHTON BROWNE, M.D. LL.D. F.R.S. Treasurer and  
Vice-President, in the Chair.

PROFESSOR G. D. LIVEING, M.A. F.R.S.

*Crystallisation.*

THERE is something very fascinating about crystals. It is not merely the intrinsic beauty of their forms, their picturesque grouping, and the play of light upon their faces, but there is a feeling of wonder at the power of nature which causes substances in passing from the fluid to the solid state to assume regular shapes bounded by plane faces, each substance with its own set of forms, with faces arranged in characteristic symmetry; some, like alum, in perfect octahedra, and others, like blue vitriol, in shapes which are regularly oblique.

It is this power of nature which will be the subject of this discourse. I hope to show that crystalline forms with all their regularity and symmetry are the outcome of accepted mechanical principles. I shall invoke no peculiar force, but only such as we are already familiar with in other natural phenomena. In fact, I shall call in only the same force that produces the rise of a liquid in a capillary tube, and the surface tension at the boundary of two substances which do not mix.

Whether this force is different from gravity I shall not stop to inquire. Any attractive force which for small masses, such as we suppose the molecules of matter to be, is only sensible at insensible distances, is sufficient for my purpose.

We know that the external form of a crystal is intimately connected with its internal structure, with the chemical nature, the arrangement and the motions, of the molecules. This internal structure betrays itself in the cleavages with which every one is familiar in mica and selenite, which extend to the minutest parts, so that when calc-spar is crushed, even the dust consists of tiny rhombs. It is still better seen in the optical characters. The regular crystals, like common salt, give no double refraction, while those less regular refract doubly, and indicate different degrees of symmetry by their action on polarised light. These familiar facts suggest that it is the internal structure which determines the external form.

As a starting-point for considering that structure I assume that crystals are made up of molecules, and that in the solid state the molecules have little freedom; that they are always within the range of each other's influence, and cannot change their relative places. Nevertheless, these molecules must be in constant and very rapid motion. Not only will they communicate heat to colder bodies which touch them, but they are always radiating, which means that they are

always producing waves in the ether at the rate of many billions a second. We are sure that they have a great deal of energy, and if they cannot move far they must have very rapid vibratory motion. It is reasonable to suppose that the parts of each molecule swing backwards and forwards through, or about, the centre of mass of the molecule. The average distance to which the parts swing will give the average dimensions of the molecule.

Dalton fancied that he had proved that the atoms of the chemical elements must be spherical, because there was no assignable cause why they should be longer in one dimension than another. I rather invert the argument. I see no reason why the excursions of the parts of a molecule from the centre of mass should be equal in every direction. I assume, as the most general case, that these excursions are unequal in different directions, and since the movements must be symmetrical with reference to the centre of mass, they will in general be included within an ellipsoid, of which the centre is the centre of mass.

Here I may, perhaps, guard against a misconception. Chemists are familiar with the notion of complex molecules, and most of us figure to ourselves a molecule of common salt as consisting of an atom of sodium and an atom of chlorine held together by some sort of force, and it may be imagined that these atoms are the parts of the molecules which I have in mind. That, however, is not my notion. I am paradoxical enough to disbelieve altogether in the existence of either sodium or chlorine in common salt. Were my audience a less philosophical one, I could imagine the retort on many a lip—"Why, you can get sodium and chlorine out of it, and you can make it out of sodium and chlorine." But, no; you cannot get either sodium or chlorine out of it without first adding something which seems to me of the essence of the matter. You can get neither sodium nor chlorine from it without adding energy. Nor can you make salt out of those elements without subtracting energy. My point is that the energy is of the essence of the molecule. Each kind of molecule has its own kind of motion; and in this I think most physicists will agree with me. The chemists will agree with me in thinking that all the molecules of the same element or compound are alike in mass, and in the space they occupy at a given temperature and pressure. The only further assumption I have to make is that the form of the ellipsoid, the relative length of its axes, is on the average the same for all the molecules of the same substance. This implies that the distances of the excursions of the parts of the molecule depend upon its constitution, and are, on the average, the same in similarly constituted molecules under similar circumstances.

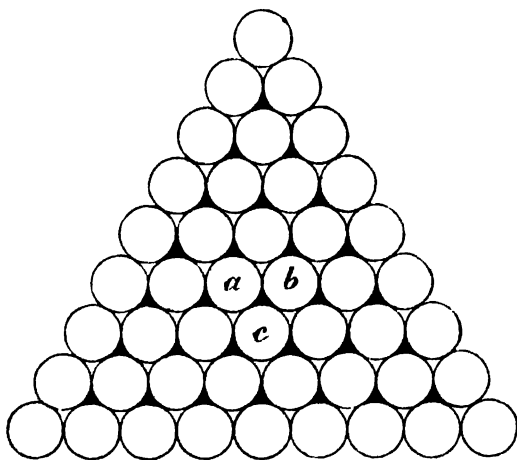
I have now come to the end of my postulates. I hope they are such as you will readily concede. I want you to conceive of each molecule that its parts are in extremely rapid vibration, so that it occupies a larger space than it would occupy if its parts were all at rest; and that the excursions of the parts about the centre of mass

are, on the average, at a given temperature and pressure, comprised within a certain ellipsoid: that the dimensions of this ellipsoid are the same for all molecules of the same chemical constitution, but different for different kinds of molecules.

We have next to consider how these molecules will pack themselves in passing from the fluid state, in which they can and do move about amongst themselves, into the solid state, in which they have no sensible freedom. If they attract one another according to any law, and for my purpose gravity will suffice, then the laws of energy require that for stable equilibrium the potential energy of the system shall be a minimum. This is the same, in the case we are considering, as saying that the molecules shall be packed in such a way that the distance between their centres of mass shall be the least possible, or as many of them as possible be packed into a given space.

In order to see how this packing will take place, it will be easiest to consider the case in which the axes of the ellipsoids are all equal—that is, when the ellipsoids happen to be spheres. The problem is then reduced to finding how to pack the greatest number of equal spherical balls into a given space. It is easy to reduce this problem to that of finding how the spheres can be arranged so that each sphere shall be touched by as many as possible of its neighbours. In this way the cornered spaces between the spheres, the spaces not occupied, are reduced to a minimum. Now, you can arrange balls so that each is touched by twelve others, but not by more than twelve. This, then, will be the arrangement which the molecules will naturally assume.

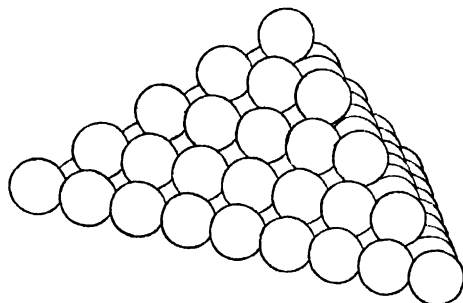
FIG. 1.



We may do this apparently in two ways. We may begin with arranging balls on a flat surface so that each is touched by six others, as in Fig. 1. We may then place a ball so that it rests on three, *a*, *b*, *c*,

in the figure, and may place six others, touching it, and resting in the six adjacent triangular spaces which are black in the figure. Above these we can again place three more so as to touch the first. If we complete the pile we get a triangular pyramid, as in Fig. 2. Or we

FIG. 2.



may begin by arranging balls on the flat, as in Fig. 3, so that each is touched by four others. We may then place one ball so as to rest on four, such as *a, b, c, d*, in the figure. Then place four others, touching it, in the four adjacent square-shaped openings which are shaded in the figure. Above these, in places corresponding to *a, b, c, d*, four more may be placed so as to touch the first. If the pile be completed it will form a four-sided pyramid, as in Fig. 4.

FIG. 3.

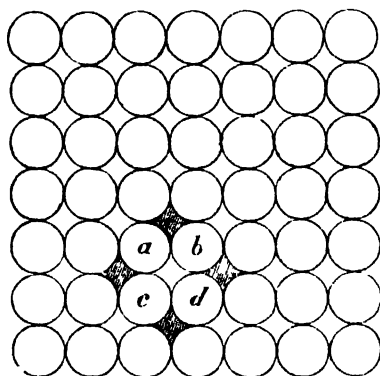
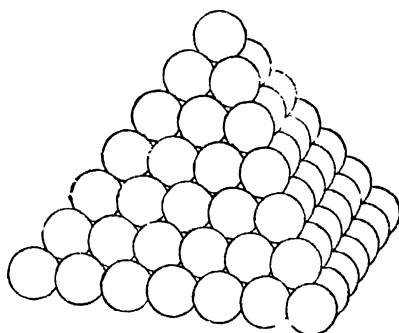


FIG. 4.



Although this arrangement seems at first sight different from that in Fig. 2, it is not so; for it will be seen that in the faces of the pyramid of Fig. 2 the arrangement is that of Fig. 3, while in the faces of the pyramid in Fig. 4 the arrangement is that of Fig. 1. Fig. 2 is really part of Fig. 4 turned over on its side.

Before proceeding to the packing of ellipsoids, let us consider

how this packing of the spheres will affect the external form. And here I must bring in the surface tension. We are familiar with the effects of this force in the case of liquids, and if we adopt the usual theory of it we must have a surface tension at the boundary of a solid as well as at the surface of a liquid. I know of no actual measures of the surface tension of solids. But Quincke has given us the surface tensions of a number of substances at temperatures near their points of solidification. The surface tension of most of the solids are probably greater than these, since surface tension usually diminishes with increase of temperature.

TABLE OF SURFACE TENSIONS OF SUBSTANCES near their Temperatures of Solidification, in dynes per lineal centimetre, after Quincke.

Platinum .. .. .	1,658	Antimony .. .. .	244
Gold .. .. .	983	Borax .. .. .	212
Zinc .. .. .	860	Sodium carbonate .. .. .	206
Tin .. .. .	587	Sodium chloride .. .. .	114
Mercury .. .. .	577	Water .. .. .	86.2
Lead .. .. .	448	Selenium .. .. .	70.4
Silver .. .. .	419	Sulphur .. .. .	41.3
Bismuth .. .. .	382	Phosphorus .. .. .	41.1
Potassium .. .. .	364	Wax .. .. .	33.4
Sodium .. .. .	253		

We have evidently to do here with an agency which we cannot neglect. In all these cases the measured tension is at a surface bounded by air, and is such as tends to contract the surface. We have then at the boundary between a crystallising solid and a fluid, gas or liquid, out of which it is solidifying, a certain amount of potential energy; and by the laws of energy the condition of equilibrium is that this potential energy shall be a minimum. The accepted theory of surface tension is that it arises from the mutual attractions of the molecules. The energy will therefore be a minimum for a surface in which the molecules are as closely set as possible.

Now if you draw any surface through a heap of spherical balls arranged so that each is touched by twelve others, you will find that the surfaces which have the greatest number of centres of the balls in unit area are all plane surfaces; and those for which the concentration is greatest are the faces of a regular octahedron, next those of a cube, next those of a rhombic dodecahedron, and so on for the other planes which follow the crystallographic law of indices. Taking the concentration in the faces of the cube as unity, those of other forms will be

Octahedron .. .. .	1.1547	Eikositessarahedron .. .. .	4083
Dodecahedron .. .. .	7071	Triakisoctahedron .. .. .	3333
Tetrakisshexahedron .. .. .	4472		

It must not be supposed that these figures give the surface energies. We have at present no means of determining the exact magnitudes of

the surface energies. What we can assert is that greater concentration means in general less surface energy.

Hence when the molecules are spherical the bounding surface tends to be that of a regular octahedron.

But we have another point to consider. Since the solid must be a closed figure, there will be edges where the bounding planes meet each other. At these edges the surface tensions will have a resultant tending to compress the crystal, and there must be a corresponding resultant pressure on the opposite side. It follows from this that if one pair of faces are developed on one side of a crystal a parallel pair must in general be developed on the opposite side, and if one face of a form, be it cube, octahedron, or other form, be developed, all the faces of that form will, as a rule, be developed.

But there is yet another point to be taken into account. The surface energy may become less in two ways; one by reducing the tension per unit of surface, and the other by reducing the total surface for the same quantity of matter. When a liquid separates from another liquid, as chloroform from a solution of chloral hydrate by adding an alkali, or a cloud from moist air, the liquid assumes the form which for a given mass has the least surface, that is the drops are spherical. If you cut off the projecting angles, and plane away the projecting edges of a cube or octahedron, you bring it nearer to a sphere, and diminish the surface per unit volume. And if diminution of the total surface is not compensated by the increase of the surface energy on the truncations, there will be a tendency for the crystal to grow with such truncations. The like will be true in more complicated combinations. There will be a tendency for such combinations to form provided the surface energy of the new faces is not too great as compared with that of the first formed faces.

But it does not always happen that an octahedron of alum developes truncated angles. This leads to another point. To produce a surface in a homogeneous mass requires a supply of energy, and to produce a surface in the interior of any fluid is not easy. Air may be supersaturated with aqueous vapour, or a solution supersaturated with a salt, and no cloud or crystals be formed in the interior, unless there is some discontinuity in the mass, specks of dust or something of the kind.

When solid matter separates from a solution, a certain amount of energy is available from the change of state, and supplies the surface energy of the new solid. The amount of this available energy is proportional to the mass of solid separated. But since the surface varies as the square of the diameter, while the mass varies as the cube of the diameter, the amount of energy available when the mass is very minute may be insufficient. In fact, a very small mass of solid might be squeezed into liquid again by its own surface energy. It will be easier to add to a surface already formed, even if that surface be one of less energy than that of the new solid, than it is to break the continuity of the fluid. Hence we find that crystals often form on the

side of the vessel, or at the top where the liquid meets the air. But it is easiest of all to add to a surface of the same energy as that of the crystal. The additional energy required will then be only for the extension of the surface. This explains why dropping a crystal into a supersaturated solution starts crystallisation. Large crystals grow more readily than small ones because the extension of surface, that is the addition of energy, for a given addition of mass is less in the former. Also it is easier to add to the faces already formed than to develop new faces.

While speaking of the difficulty of creating a new surface in the interior of a mass, the question of cleavage suggests itself. It is plain that in dividing a crystal we create a new surface on each of the two parts, each with its own surface energy. The division must therefore take place most readily where that surface energy is a minimum. Hence I infer that the principal cleavage of a crystal made up of molecules for which the vibrations are comprised within spherical spaces will be octahedral. As a fact, we find that the greater part of substances which crystallise in what is called the regular system, have an octahedral cleavage. But not all; there are some which have a cubical cleavage such as rock salt and galena, and a very few like blende have the principal cleavage dodecahedral. These I have to explain.

I may, however, first observe that some substances, like fluor spar, which have a very distinct octahedral cleavage, are rarely met with in octahedral crystals, but usually in the cubic form. In regard to this we must remember that the surface energy depends upon the nature of both the substances which meet at the common surface, their electrical state, their temperature, and other circumstances. It is a well known fact that the form assumed by a salt on crystallising is affected by the character of the solution. Thus, alum, which from a solution in pure water takes the octahedral form, from a solution neutralised with potash takes the cubic form. It is therefore quite possible that, under the circumstances in which the natural crystals of fluor spar were formed, the surface energy of the cubical faces was less than that of the octahedral, although when we experiment upon them in the air it is the other way. The closeness of the molecules in the surface of the solid will determine the surface energy so far as the solid alone is concerned; but though this may be the most important factor of the result, the molecules of the fluid in contact with the crystal have their effect too.

But to return to cubic and dodecahedral cleavages. If we suppose the excursions of the parts of the molecule to be greater in some one direction than in the others, the figure within which the molecule vibrates will be a prolate spheroid; if it be less, an oblate spheroid. Now if such spheroids be packed as close as possible, each can be touched by twelve others, and they can be packed just as the spheres were, provided their axes be all parallel. It matters not what the orientation of the axes may be so far as the closeness of packing goes,



so long as their parallelism is maintained ; but the orientation will affect very much the symmetry of the crystal.

If we suppose the spheroids to be oblate, and arrange them as in Figure 1, with their axes perpendicular to the plane of that figure, and place the next layer in those triangular openings which are white in the figure, and complete the pyramid, the magnitude of the three angles at the apex of the pyramid will depend on the relative flatness of the spheroids. In case the length of the axis of the spheroids is half their greatest diameter, these three angles will be right angles, and the whole heap of molecules will have a cubic symmetry, and in the faces of the cubes the concentration will be a maximum, and therefore the surface energy a minimum, and the easiest cleavage will be cubic. If the concentration in the cubic faces be 1.0000, that in the octahedral faces will be 0.5774, and that in the dodecahedral 0.7071. We have here the case of crystals like rock salt and galena. Suppose, however, we start with the arrangement of Figure 3, and keep the axes perpendicular to the plane of that figure ; and suppose, further, that the biggest diameter of the spheroids is greater than the length of the axis in the ratio of the diagonal to the side of a square, we shall again get a heap with a cubic symmetry ; but in this case the maximum concentration will be in the faces of the dodecahedron, and we have the case of blende in which the easiest cleavage is dodecahedral.

In order to see what the symmetry will be in other cases, we may look at the problem from another point of view. Suppose a cube made up of spherical molecules to be subject to a uniform stress perpendicular to one face of the cube, so that all the spheres are strained, either by extension or compression, into spheroids, we should get that diagonal of the octahedron which was parallel to the stress either lengthened or shortened, but the symmetry about that diagonal would remain as before. We should get a crystal of the pyramidal system. If the spheroids were prolate and sufficiently elongated, the easiest cleavage would be perpendicular to the axis as in potassium ferrocyanide and apophyllite. If the spheroids were oblate the fundamental octahedron would be more obtuse, and if obtuse enough the easiest cleavage would be in faces parallel to the axis of symmetry.

Again, if the stress, instead of being perpendicular to one face of the cube, were parallel to a diagonal of the cube, the cube would become a rhombohedron, and the spheres would become spheroids with their axes parallel to the axis of the rhombohedron. If the spheroids were prolate the rhombohedron would be acute, and the easiest cleavage perpendicular to the axis as we find it in beryl and many other crystals. If the original cube were formed of spheroids with their axes half the length of their greatest diameters, and the stress parallel to the axes were such as to alter the length of the axes only a little, we should get crystals with a rhombohedral cleavage like calcite. The crystals like beryl almost always exhibit hexagonal forms, six-sided prisms and pyramids. To explain this I would observe that if

we start with spheroids arranged as in Figure 1, with their axes perpendicular to the plane of that figure, and place three others touching that marked  $a$ ; there are two ways in which we can do this. We may place the three either in the white or in the black triangles. The two positions differ in such a way that you pass from one to the other by turning the three spheroids as a whole through  $180^\circ$ . The relation is that of twin crystals. If a crystal were growing by addition to the face which we suppose represented in Figure 1, it would be as likely that one arrangement should be taken as the other so far as the middle part of the face is concerned. But a crystal built up of such alternate layers of twins would have ridged and furrowed faces, that is faces of extra surface-tension, except in the case of hexagonal forms. For hexagonal forms are no way altered by being turned through two right angles. There will therefore be a tendency for such forms to grow unless the rhombohedral faces have a much less surface energy. Hexagonal forms have also less surface per unit of volume than rhombohedrons, and lend themselves to the formation of nearly globular crystals, with a minimum of total surface, as is often seen in pyromorphite.

Recurring to the cube of spheres, if it be subject to a stress in a direction not parallel to an edge or diagonal, we shall get an arrangement of spheroids which will give forms of less symmetry. Also if it be subject to two uniform stresses at right angles to one another the spheres will become ellipsoids and may be taken to represent the molecules in the most general case. The degrees of symmetry, and the directions of most easy cleavage, may be worked out on the lines already indicated, and will be found to correspond with those observed in nature.

Bravais long ago suggested arrangements of the molecules corresponding to the symmetry, and Sohncke has extended his suggestions, but neither has assigned any mechanical reason why the molecules should so arrange themselves. They also supposed different arrangements for different kinds of symmetry. I have endeavoured to give a sufficient reason for the positions taken by the molecules and to show that out of the one arrangement by which the molecules are packed as closely as is possible all the varieties of symmetry will arise.

M. Curie also has, before me, pointed out that differences of surface tension will determine the relative sizes of different faces; but he has not pointed out that the same principle determines that the faces shall be planes, and that similar edges and angles shall be similarly modified, or that the law of indices in the relations of different forms is a direct consequence of it.

We are able now, I think, to understand the interesting facts brought forward by Prof. Judd in a discourse which he delivered at the Royal Institution in the early part of this year.

It does not matter how long a crystal has been out of the solution or vapour in which it was formed, the surface tension remains

the same, and it must grow on its old faces if replaced in the same medium.

Also if it have any part broken off, the tension of the broken surface will, if it be not a cleavage face, be greater than on a face of the crystal, and in growing, the laws of energy necessarily cause it to grow in such a way as to reduce the potential energy to a minimum, i. e. to replace the broken surface by the regular planes of less surface energy.

The formation of what have been called "negative crystals" by fusion in the interior of a mass, is due to the same principle. If the mass is crystalline in structure the surfaces of least energy will be most easily produced in the inside as well as on the outside.

We see a very similar result in the development of crystalline form by the action of solvents, as of acids on metals. The substance acted on must be crystalline in its molecular arrangement internally, though its external figure may have been derived from the shape of the vessel or other cause. If this is the case, and if the acid is not so strong as to dissolve the metal rapidly, there must be a tendency for those parts of the surface for which the energy is greatest to be most easily removed. The result is to leave a crystalline form with surfaces of minimum energy, as we see in Widmanstatt figures, a tin plate acted on by dilute aqua regia, and many such cases.

In fact, the solution of solids in liquids is very closely and directly connected with the surface tension. One of many facts connected with crystallisation is that the same substance in one crystalline form may be soluble in a liquid in which it is not soluble when it has another crystalline form. It is probably the low surface energy of one form of crystals of sulphur which makes them insoluble in carbon disulphide, and this low surface energy may be an electric effect. It is not difficult to understand that the same molecules may give rise to crystals of different degrees of symmetry, according to the orientation of the axes, and the orientation of the axes may very well depend on the distribution of the mass within the molecule, or the molecules may in one case contain a greater number of chemical atoms than in the other. With different crystalline forms of the same kind of substance we shall in general have different surface energies, and a surface of great energy will be attacked by a solvent when one of less energy will resist it.

I pointed out that the law of symmetry, the development of all the faces of any form, and the similar modification of all corresponding edges and angles, is in general necessary in order to give equilibrium under the action of the surface tensions. But we often find crystals with only half the modifications required for symmetry. In such cases the surface tensions must produce a stress in the interior tending to deform the crystal. When the crystal was in process of formation there was necessarily equilibrium, and there must have been a pressure equal and opposite to this effect of the surface tension. There are various ways in which we may suppose that such a force

would arise; for instance, the electric field might produce a stress in opposition to the aggregation of the molecules in the closest possible way, and then the crystal would develop such faces as would give rise to an equal and opposite stress. The presence of molecules of some other material, different from those forming the bulk of the crystal, might cause a similar effect, so might inequalities of temperature.

In the case of an electric stress, or one due to inequalities of temperature, when the electric stress, or the inequality of temperature was removed, the crystal would be left with an internal strain, because the stress due to the want of symmetry must be met by an equal pressure.

Crystals of this sort generally betray the internal strain, either by developing electricity of opposite kinds at the two ends when they are heated, or cooled, or they affect polarised light, rotating the plane of polarisation. That these effects are really due to the state of internal strain is proved because tourmalines, and other crystals, which are pyro-electric when unsymmetrical, show no such property when symmetrically grown; and sodium chlorate when in solution, quartz when fused, and so on, lose their rotatory power. On the other hand there are many substances which in solution show a rotatory power, and as a rule such substances produce unsymmetrically developed crystals. This is well seen in the tartrates. The constitution of the molecules must naturally be such that they will not, without some strain, form crystals, and equilibrium in the crystal is attained by the opposing stress arising from want of symmetry in the surface tensions. In all such crystals the rotatory power disappears either in whole or in part when the substance crystallises. It is impossible however, in biaxial crystals to tell whether there is rotation or not. According to Des Cloizeaux the only crystal formed from a liquid having rotatory power, which shows rotation in the solid state, is strychnine sulphate. This substance forms crystals like prussiate of potash, double square pyramids with the two apices truncated. Its rotatory power in the crystalline form is much stronger than in the liquid form. The crystals are not hemihedral, and the rotatory power is not due to any stress arising from want of symmetry in surface tensions. Effects more or less analogous to those due to the stress arising from unsymmetric development may be produced in crystals by external pressure. Thus a piece of rock salt, which in its natural state has no action on polarised light, when compressed in a vice will change the plane of polarisation. Also a cleavage slice of potassium ferrocyanide which is uniaxial, may, by compression, be made to give in convergent polarised light the two eyes and elliptic rings of a biaxial crystal.

Fig 4

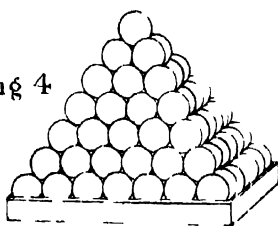


Fig 5

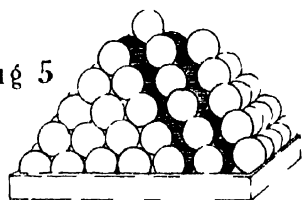


Fig 2

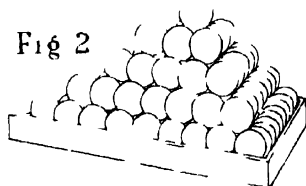


Fig 9

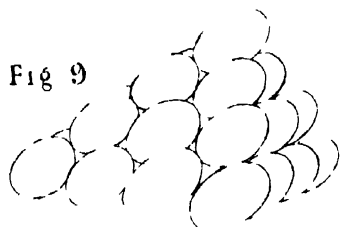


Fig 6

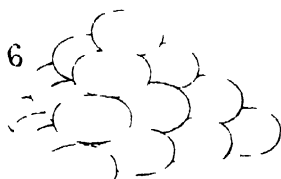


Fig 7

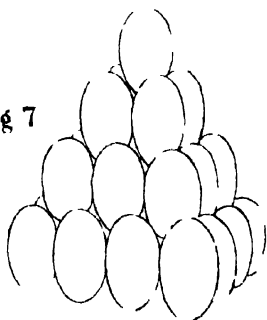


Fig 8



## EXPLANATION OF THE PLATE.

Fig. 4 shows half of a regular octahedron formed of a pile of spherical balls, and Fig. 5 shows part of a face of a dodecahedron produced by truncating one edge of Fig. 4. In this it is seen that in the plane of the dodecahedral face each ball is touched by only two others.

Fig. 6 shows the triangular pyramid formed of oblate spheroids, which becomes one corner of a cube when the ratio of the diameters of the spheroids is 2:1, but one corner of a rhombohedron if the ratio is greater or less.

Figs. 7, 8, and 9 represent halves of octahedra formed of prolate spheroids. In Fig. 7 the axes are perpendicular to the base, and the octahedron has pyramidal symmetry. In Fig. 8 the axes are parallel to one edge of the base, and the octahedron has right prismatic symmetry. In Fig. 9 the axes are in planes parallel to one edge of the base, but inclined to that edge, and the octahedron is oblique.

[G. D. L.]

Friday, May 22, 1891.

DAVID EDWARD HUGHES, Esq. F.R.S. Vice-President, in the Chair.

PROFESSOR J. A. EWING, M.A. B.Sc. F.R.S.

PROFESSOR OF APPLIED MECHANICS AND MECHANISM IN THE UNIVERSITY OF CAMBRIDGE.

*The Molecular Process in Magnetic Induction.*

MAGNETIC induction is the name given by Faraday to the act of becoming magnetised, which certain substances perform when they are placed in a magnetic field. A magnetic field is the region near a magnet, or near a conductor conveying an electric current. Throughout such a region there is what is called magnetic force, and when certain substances are placed in the magnetic field the magnetic force causes them to become magnetised by magnetic induction. An effective way of producing a magnetic field is to wind a conducting wire into a coil, and pass a current through the wire. Within the coil we have a region of comparatively strong magnetic force, and when a piece of iron is placed there it may be strongly magnetised. Not all substances possess this property. Put a piece of wood or stone or copper or silver into the field, and nothing noteworthy happens; but put a piece of iron or nickel or cobalt and at once you find that the piece has become a magnet. These three metals, with some of their alloys and compounds, stand out from all other substances in this respect. Not only are they capable of magnetic induction—of becoming magnets while exposed to the action of the magnetic field—but when withdrawn from the field they are found to retain a part of the magnetism they acquired. They all show this property of retentiveness, more or less. In some of them this residual magnetism is feebly held, and may be shaken out or otherwise removed without difficulty. In others, notably in some steels, it is very persistent, and the fact is taken advantage of in the manufacture of permanent magnets, which are simply bars of steel, of proper quality, which have been subjected to the action of a strong magnetic field. Of all substances, soft iron is the most susceptible to the action of the field. It can also, under favourable conditions, retain, when taken out of the field, a very large fraction of the magnetism that has been induced—more than nine-tenths—more, indeed, than is retained by steel; but its hold of this residual magnetism is not firm, and for that reason it will not serve as a material for permanent magnets. My purpose to-night is to give some account of the molecular process through which we may conceive magnetic induction to take place, and of the structure which makes residual magnetism possible.

When a piece of iron or nickel or cobalt is magnetised by induction, the magnetic state permeates the whole piece. It is not a superficial change of state. Break the piece into as many fragments as you please, and you will find that every one of these is a magnet. In seeking an explanation of magnetic quality we must penetrate the innermost framework of the substance—we must go to the molecules.

Now, in a molecular theory of magnetism there are two possible beginnings. We might suppose, with Poisson, that each molecule becomes magnetised when the field begins to act. Or we may adopt the theory of Weber, which says that the molecules of iron are always magnets, and that what the field does is to turn them so that they face more or less one way. According to this view, a virgin piece of iron shows no magnetic polarity, not because its molecules are not magnets, but because they lie so thoroughly higgledy-piggledy as regards direction that no greater number point one way than another. But when the magnetic force of the field begins to act, the molecules turn in response to it, and so a preponderating number come to face in the direction in which the magnetic force is applied, the result of which is that the piece as a whole shows magnetic polarity. All the facts go to confirm Weber's view. One fact in particular I may mention at once—it is almost conclusive in itself. When the molecular magnets are all turned to face one way, the piece has clearly received as much magnetisation as it is capable of. Accordingly, if Weber's theory be true, we must expect to find that in a very strong magnetic field a piece of iron, or other magnetisable metal, becomes *saturated*, so that it cannot take up any more magnetism, however much the field be strengthened. This is just what happens: experiments were published a few years ago which put the fact of saturation beyond a doubt, and gave values of the limit to which the intensity of magnetisation may be forced.

When a piece of iron is put in a magnetic field, we do not find that it becomes saturated unless the field is exceedingly strong. A weak field induces but little magnetism; and if the field be strengthened, more and more magnetism is acquired. This shows that the molecules do not turn with perfect readiness in response to the deflecting magnetic force of the field. Their turning is in some way resisted, and this resistance is overcome as the field is strengthened, so that the magnetism of the piece increases step by step. What is the directing force which prevents the molecules from at once yielding to the deflecting influence of the field, and to what is that force due? And again, how comes it that after they have been deflected they return partially, but by no means wholly, to their original places when the field ceases to act?

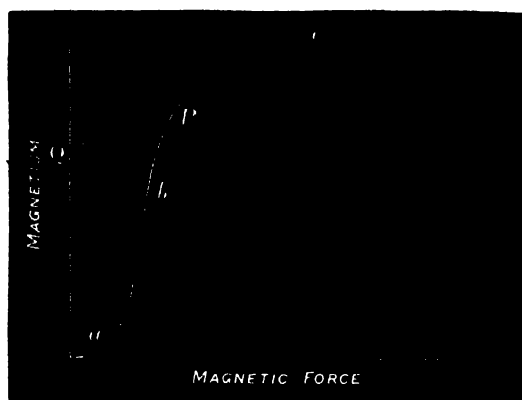
I think these questions receive a complete and satisfactory answer when we take account of the forces which the molecules necessarily exert on one another in consequence of the fact that they are magnets. We shall study the matter by examining the behaviour of



groups of little magnets, pivoted like compass needles, so that each is free to turn except for the constraint which it suffers on account of the presence of its neighbours.

But first let us see more particularly what happens when a piece of iron or steel or nickel or cobalt is magnetised by means of a field the strength of which is gradually augmented from nothing. We may make the experiment by placing a piece of iron in a coil, and making a current flow in the coil with gradually increased strength, noting at each stage the relation of the induced magnetism to the strength of the field. This relation is observed to be by no means a simple one: it may be represented by a curve (Fig. 1), and an

FIG. 1.



inspection of the curve will show that the process is divisible, broadly, into three tolerably distinct stages. In the first stage (*a*) the magnetism is being acquired but slowly: the molecules, if we accept Weber's theory, are not responding readily—they are rather hard to turn. In the second stage (*b*) their resistance to turning has to a great extent broken down, and the piece is gaining magnetism fast. In the third stage (*c*) the rate of increment of magnetism falls off: we are there approaching the condition of saturation, though the process is still a good way from being completed.

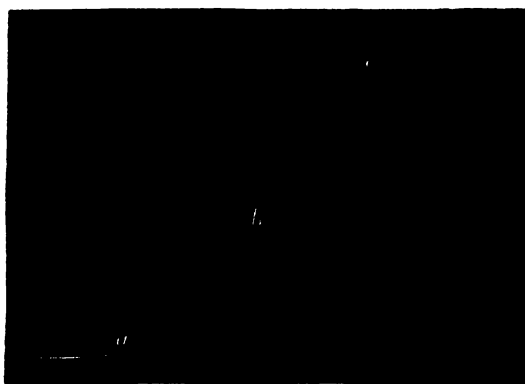
Further, if we stop at any point of the process, such as *P*, and gradually reduce the current in the coil until there is no current, and therefore no magnetic field, we shall get a curve like the dotted line *PQ*, the height of *Q* showing the amount of the residual magnetism.

If we make this experiment at a point in the first stage (*a*), we shall find, as Lord Rayleigh has shown, little or no residual magnetism; if we make it at any point in the second stage (*b*), we shall find very much residual magnetism; and if we make it at any point

in the third stage (*c*), we shall find only a little more residual magnetism than we should have found by making the experiment at the end of stage *b*. That part of the turning of the molecules which goes on in stage *a* contributes nothing to the residual magnetism. That part which goes on in stage *c* contributes little. But that part of the turning which goes on in stage *b* contributes very much.

In some specimens of magnetic metal we find a much sharper separation of the three stages than in others. By applying strain in certain ways it is possible to get the stages very clearly separated. Fig. 2, a beautiful instance of that, is taken from a paper by Mr.

FIG 2.



Nagaoka—one of an able band of Japanese workers who are bidding fair to repay the debt that Japan owes for its learning to the West. It shows how a piece of nickel which is under the joint action of pull and twist becomes magnetised in a growing magnetic field. There the first stage is exceptionally prolonged, and the second stage is extraordinarily abrupt.

The bearing of all this on the molecular theory will be evident when we turn to these models, consisting of an assemblage of little pivoted magnets, which may be taken to represent, no doubt in a very crude way, the molecular structure of a magnetisable metal. I have here some large models, where the pivoted magnets are pieces of sheet steel, some cut into short flat bars, others into diamond shapes with pointed ends, others into shapes resembling mushrooms or umbrellas, and in these the magnetic field is produced by means of a coil of insulated wire wound on a large wooden frame below the magnets. Some of these are arranged with the pivots on a gridiron or lazy-tongs of jointed wooden bars, so that we may readily distort them, and vary the distances of the pivots from one another, to imitate some of the effects of strain in the actual solid. But to display the experiments to a large audience a lantern model will serve best. In this one the

magnets are got by taking to pieces numbers of little pocket compasses. The pivots are cemented to a glass plate, through which the light passes in such a way as to project the shadows of the magnets on the screen. The magnetic force is applied by means of two coils, one on either side of the assemblage of magnets and out of the way of the light, which together produce a nearly uniform magnetic field throughout the whole group. You see this when I make manifest the field in a well-known fashion, by dropping iron filings on the plate.

We shall first put a single pivoted magnet on the plate. So long as no field acts it is free to point anyhow—there is no direction it prefers to any other. As soon as I apply even a very weak field it responds, turning at once into the exact direction of the applied force, for there was nothing (beyond a trifling friction at the pivot) to prevent it from turning.

Now try two magnets. I have cut off the current, so that there is at present no field, but you see at once that the pair has, so to speak, a will of its own. I may shake or disturb them as I please, but they insist on taking up a position (Fig. 3) with the north end of one as close as possible to the south end of the other. If disturbed they return to it: this configuration is highly stable. Watch what happens when the magnetic field acts with gradually growing strength. At first, so long as the field is weak (Fig 4), there is but little deflection ;

FIG. 3.

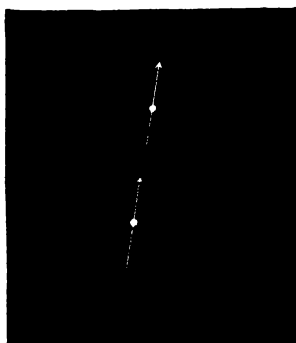
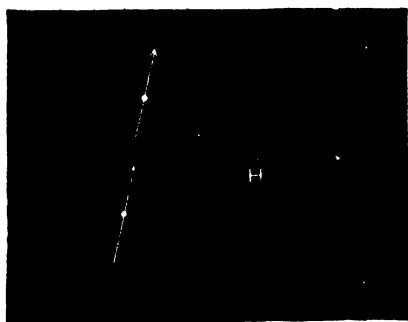


FIG 4.



but as the deflection increases it is evident that the stability is being lost, the state is getting more and more critical, until (Fig. 5) the tie that holds them together seems to break, and they suddenly turn, with violent swinging, into almost perfect alignment with the magnetic force H. Now I gradually remove the force, and you see that they are slow to return, but a stage comes when they swing back, and a complete removal of the force brings them into the condition with which we began (Fig. 3).

If we were to picture a piece of iron as formed of a vast number of such pairs of molecular magnets, each pair far enough from its neigh-

bours to be practically out of reach of their magnetic influence, we might deduce many of the observed magnetic properties, but not all. In particular, we should not be able to account for so much residual magnetism as is actually found. To get that, the molecules must

FIG. 5.



FIG. 6.



make new connections when the old ones are broken; their relations are of a kind more complex than the quasi-matrimonial one which this experiment exhibits. Each molecule is a member of a larger community, and has probably many neighbours close enough to affect its conduct.

We get a better idea of what happens by considering four magnets (Fig. 6). At first, in the absence of deflecting magnetic force, they group themselves in stable pairs—in one of a number of possible

FIG. 7.



combinations. Then—as in the former case—when magnetic force is applied, they are at first slightly deflected, in a manner that exactly tallies with what I have called the stage *a* of the magnetising process. Next comes instability. The original ties break up, and the magnets

swing violently round; but finding a new possibility of combining (Fig. 7), they take to that. Finally, as the field is further strengthened

FIG. 8.

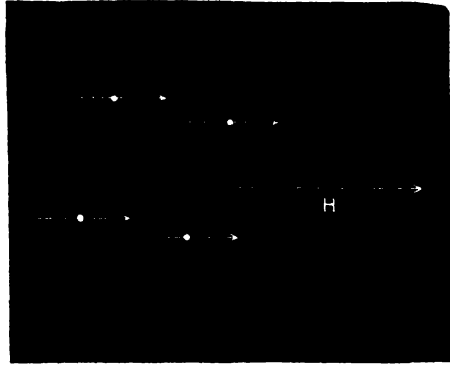
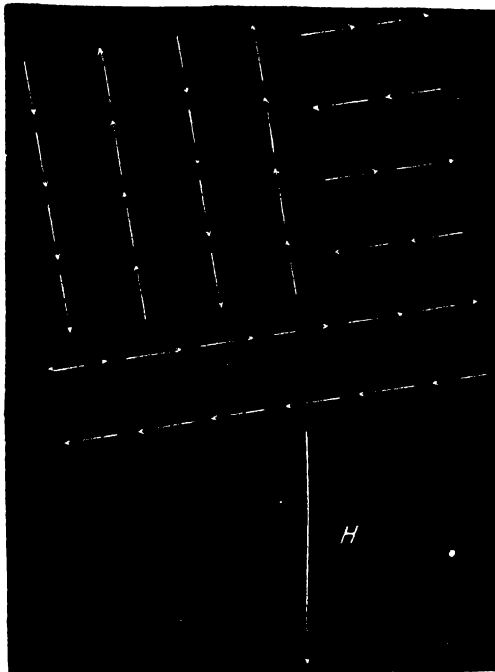


FIG. 9.



they are drawn into perfect alignment with the applied magnetic force (Fig. 8).

We see the same three stages in a multiform group (Figs, 9, 10, 11).

At first, the group, if it has been shuffled by any casual disturbance, arranges itself at random in lines that give no resultant polarity. A weak force produces no more than slight quasi-elastic deflections; a stronger force breaks up the old lines, and forms new ones more favourably inclined to the direction of the force (Fig. 10). A very strong force brings about saturation (Fig. 11).

In an actual piece of iron there are multitudes of groups lying variously directed to begin with—perhaps also different as regards the spacing of their members. Some enter the second stage while

FIG 10

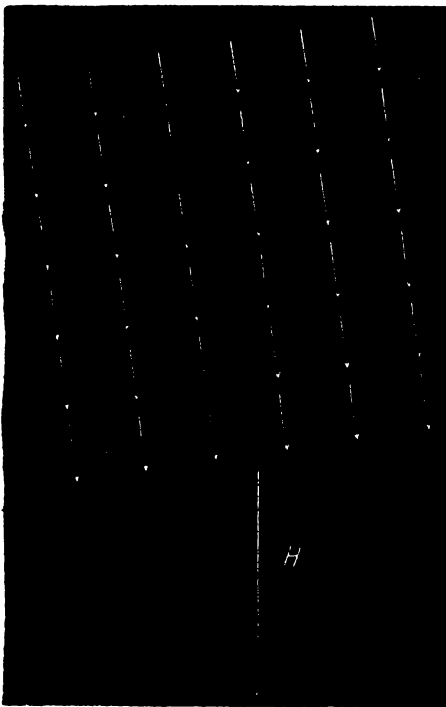
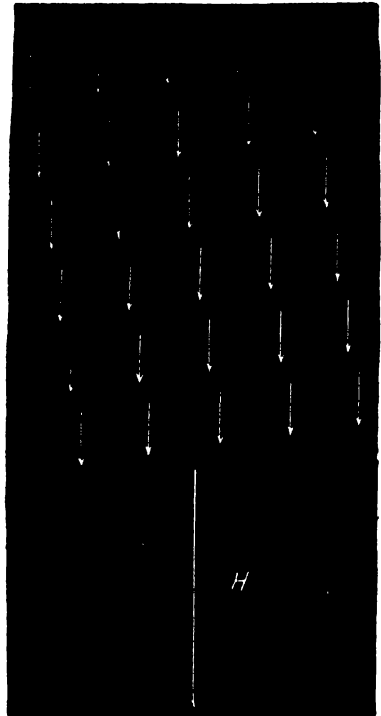


FIG 11.



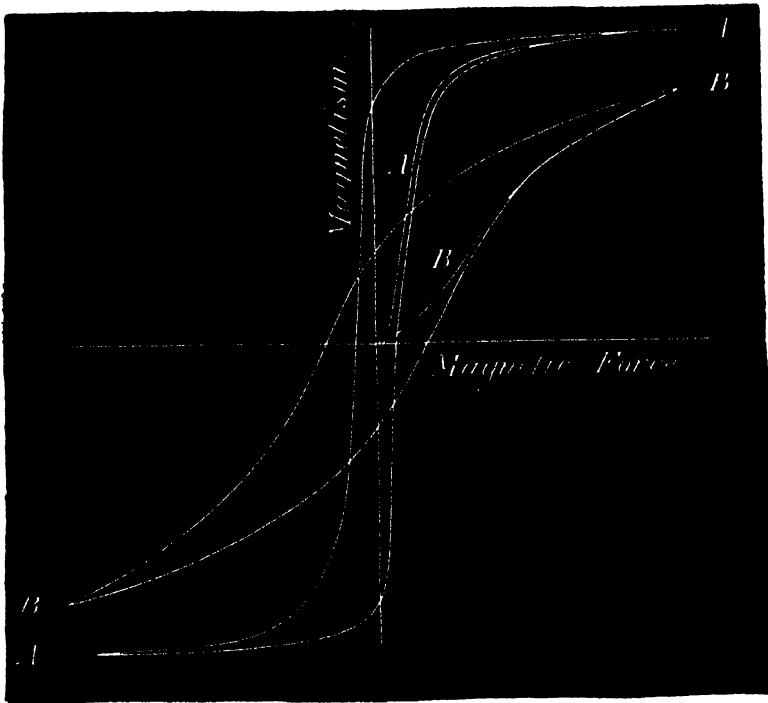
others are still in the first, and so on. Hence, the curve of magnetisation does not consist of perfectly sharp steps, but has the rounded outlines of Fig. 1.

Notice, again, how the behaviour of these assemblages of elementary magnets agrees with what I have said about residual magnetism. If we stop strengthening the field before the first stage is passed—before any of the magnets have become unstable and have tumbled round into new places—the small deflection simply disappears, and there is no residual effect on the configuration of the

group. But if we carry the process far enough to have unstable deflections, the effects of these persist when the force is removed, for the magnets then retain the new grouping into which they have fallen (Fig. 10). And again, the quasi-elastic deflections which go on during the third stage do not add to the residual magnetism.

Notice, further, what happens to the group if after applying a magnetic force in one direction and removing it, I begin to apply force in the opposite direction. At first there is little reduction of the residual polarity till a stage is reached when instability begins,

FIG. 12.



Cyclic reversal of magnetisation in (A A) annealed iron wire, (B B) the same piece hardened by stretching.

and then reversal occurs with a rush. We thus find a close imitation of all the features that are actually observed when iron or any of the other magnetic metals is carried through a cyclic magnetising process (Fig. 12). The effect of any such process is to form a *loop* in the curve which expresses the relation of the magnetism to the magnetising force. The changes of magnetism always lag behind the

changes of magnetising force. This tendency to lag behind is called magnetic *hysteresis*.

We have a manifestation of hysteresis whenever a magnetic metal has its magnetism changed in any manner through changes in the magnetising force, unless, indeed, the changes are so minute as to be confined to what I have called the first stage (*a*, Fig. 1). Residual magnetism is only a particular case of hysteresis.

Hysteresis comes in whatever be the character or cause of the magnetic change, provided it involves such deflections on the part of the molecules as make them become unstable. The unstable movements are not reversible with respect to the agent which produces them—that is to say, they are not simply undone step by step as the agent is removed.

We know, on quite independent grounds, that when the magnetism of a piece of iron or steel is reversed, or indeed cyclically altered in any way, some work is spent in performing the operation—energy is being given to the iron at one stage, and is being recovered from it at another; but when the cycle is taken as a whole, there is a net loss, or rather a waste of energy. It may be shown that this waste is proportional to the area of the loop in our diagrams. This energy is dissipated; that is to say, it is scattered and rendered useless: it takes the form of heat. The iron core of a transformer, for instance, which is having its magnetism reversed with every pulsation of the alternating current, tends to become hot for this very reason; indeed, the loss of energy which happens in it, in consequence of magnetic hysteresis, is a serious drawback to the efficiency of alternating-current systems of distributing electricity. It is the chief reason why they require much more coal to be burnt, for every unit of electricity sold, than direct-current systems require.

The molecular theory shows how this waste of energy occurs. When the molecule becomes unstable and tumbles violently over, it oscillates and sets its neighbours oscillating, until the oscillations are damped out by the eddy currents of electricity which they generate in the surrounding conducting mass. The useful work that can be got from the molecule as it falls over is less than the work that is done in replacing it during the return portion of the cycle. This is a simple mechanical deduction from the fact that the movement has unstable phases.

I cannot attempt, in a single lecture, to do more than glance at several places where the molecular theory seems to throw a flood of light on obscure and complicated facts, as soon as we recognise that the constraint of the molecules is due to their mutual action as magnets.

It has been known since the time of Gilbert that vibration greatly facilitates the process of magnetic induction. Let a piece of iron be briskly tapped while it lies in the magnetic field, and it is found to take up a large addition to its induced magnetism. Indeed, if we examine the successive stages of the process while the iron is kept vibrating by being tapped, we find that the first stage (*a*) has practi-

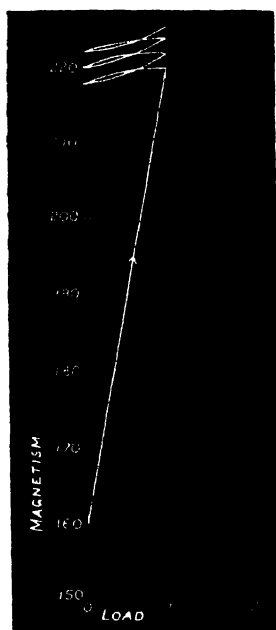


cally disappeared, and there is a steady and rapid growth of magnetism almost from the very first. This is intelligible enough. Vibration sets the molecular magnets oscillating, and allows them to break their primitive mutual ties and to respond to weak deflecting forces. For a similar reason vibration should tend to reduce the residue of magnetism which is left when the magnetising force is removed, and this, too, agrees with the results of observation.

Perhaps the most effective way to show the influence of vibration is to apply a weak magnetising force first, before tapping. If the force is adjusted so that it nearly but not quite reaches the limit of stage (a), a great number of the molecular magnets are, so to speak, hovering on the verge of instability, and when the piece is tapped they go over like a house of cards, and magnetism is acquired with a rush. Tapping always has some effect of the same kind, even though there has been no special adjustment of the field.

And other things besides vibration will act in a similar way, precipitating the break-up of molecular groups when the ties are already strained. Change of temperature will sometimes do it, or the application or change of mechanical strain. Suppose, for instance, that we apply pull to an iron wire while it hangs in a weak magnetic field, by making it carry a weight. The first time that we put on the weight, the magnetism of the wire at once increases, often very greatly, in consequence of the action I have just described (Fig. 13). The molecules have been on the verge of turning, and the slight strain caused by the weight is enough to make them go. Remove the weight, and there is only a comparatively small change in the magnetism, for the greater part of the molecular turning that was done when the weight was put on is not undone when it is taken off. Reapply the weight, and you find again but little change, though there are still traces of the kind of action which the first application brought about. That is to say, there are some groups of molecules which, though they were not broken up in the first application of the weight, yield now, because they have lost the support they then obtained from neighbours that have now entered into new combinations. Indeed, this kind of action may often be traced, always diminishing in amount, during several

FIG. 13.



Effects of loading, unloading, and reloading a soft iron wire in a weak magnetic field.

successive applications and removals of the load (see Fig. 13), and it is only when the process of loading has been many times repeated that the magnetic change brought about by loading is just opposite to the magnetic change brought about by unloading.

Whenever, indeed, we are observing the effects of an alteration of physical condition on the magnetism of iron, we have to distinguish between the primitive effect, which is often very great and is not reversible, and the ultimate effect, which is seen only after the molecular structure has become somewhat settled through many repetitions of the process. Experiments on the effects of temperature, of strain, and so forth, have long ago shown this distinction to be exceedingly important: the molecular theory makes it perfectly intelligible.

Further, the theory makes plain another curious result of experiment. When we have loaded and unloaded the iron wire many times over, so that the effect is no longer complicated by the primitive action I have just described, we still find that the magnetic changes which occur while the load is being put on are not simply undone, step by step, while the load is being taken off. Let the whole load be divided into several parts, and you will see that the magnetism has two different values, in going up and in coming down, for one and the same intermediate value of the load. The changes of magnetism lag behind the changes of load: in other words, there is hysteresis in the relation of the magnetism to the load (Fig. 14). This is because some of the molecular groups are every time being broken up during the loading, and re-established during the unloading, and that, as we saw already, involves hysteresis. Consequently, too, each loading and unloading requires the expenditure of a small quantity of energy, which goes to heat the metal.

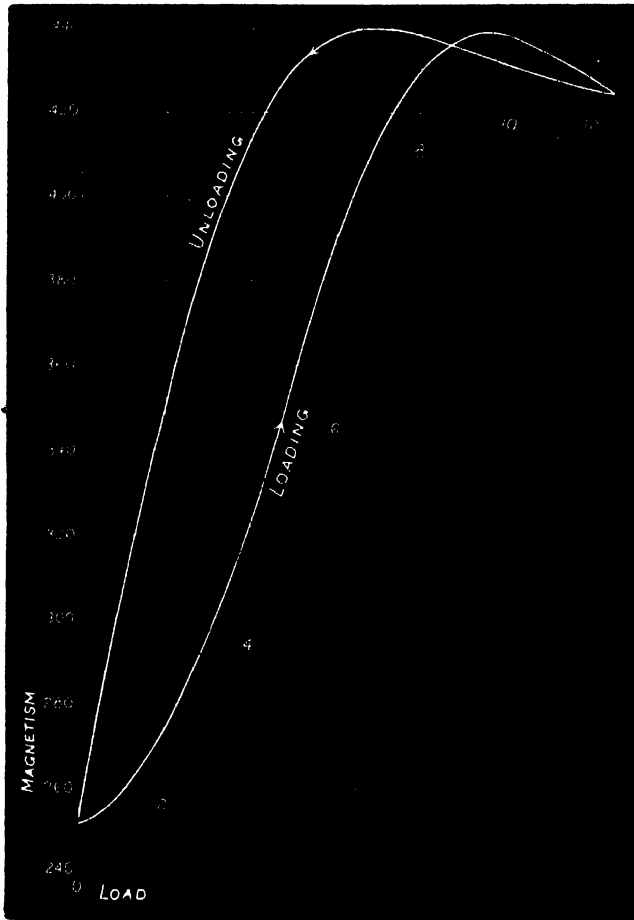
Moreover, a remarkably interesting conclusion follows. This hysteresis, and consequent dissipation of energy, will also happen though there be no magnetisation of the piece as a whole: it depends on the fact that the molecules are magnets. Accordingly, we should expect to find—and experiment confirms this\*—that if the wire is loaded and unloaded, even when no magnetic field acts and there is no magnetism, its physical qualities which are changed by the load will change in a manner involving hysteresis. In particular, the length must be less for the same load during loading than during unloading so that work may be wasted in every cycle of loads. There can be no such thing as perfect elasticity in a magnetisable metal, unless, indeed the range of the strain is so very narrow that none of the molecules tumble through unstable states. This may have something to do with the fact, well known to engineers, that numerous repetitions of a straining action, so slight as to be safe enough in itself, have a dangerous effect on the structure of iron or steel.

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\* See Phil. Trans. 1885, p. 614.

Another thing on which the theory throws light is the phenomenon of time-lag in magnetisation. When a piece of iron is put into a steady magnetic field, it does not take instantly all the magnetism

FIG. 14.



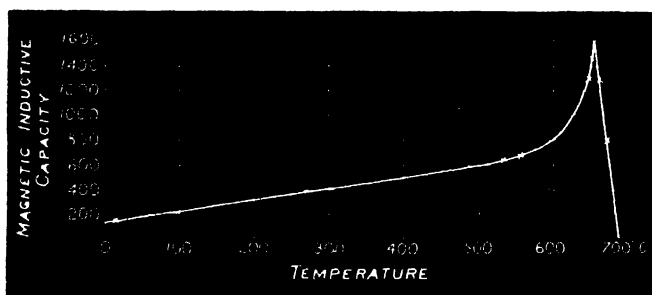
Hysteresis in the changes of magnetism produced by applying and removing load.

that it will take if time be allowed. There is a gradual creeping up of the magnetism, which is most noticeable when the field is weak and when the iron is thick. If you will watch the manner in which a group of these little magnets breaks up when a magnetic force is applied to it, you will see that the process is one that takes time. The first molecule to yield is some outlying one which is comparatively unattached—as we may take the surface molecules in the piece of iron

to be. It falls over, and then its neighbours, weakened by the loss of its support, follow suit, and gradually the disturbance propagates itself from molecule to molecule throughout the group. In a very thin piece of iron—a fine wire, for instance—there are so many surface molecules, in comparison with the whole number, and consequently so many points which may become origins of disturbance, that the breaking up of the molecular communities is too soon over to allow much of this kind of lagging to be noticed.

Effects of temperature, again, may be interpreted by help of the molecular theory. When iron or steel or nickel is heated in a weak magnetic field, its susceptibility to magnetic induction is observed to increase, until a stage is reached, at a rather high temperature, when the magnetic quality vanishes almost suddenly and almost completely. Fig. 15, from one of Hopkinson's papers, shows what is observed as the temperature of a piece of steel is gradually raised. The sudden loss of magnetic quality occurs when the metal has become red-hot; the

FIG. 15.



Effects of rising temperature on the magnetic inductive capacity of steel (Hopkinson).

magnetic quality is recovered when it cools again sufficiently to cease to glow. Now, as regards the first effect—the increase of susceptibility with increase of temperature—I think that is a consequence of two independent effects of heating. The structure is expanded, so that the molecular centres lie further apart. But the freedom with which the molecules obey the direction of any applied magnetic force is increased not by that only, but perhaps even more by their being thrown into vibration. When the magnetic field is weak heating consequently assists magnetisation, sometimes very greatly, by hastening the passage from stage *a* to stage *b* of the magnetising process. And it is at least a conjecture worth consideration whether the sudden loss of magnetic quality at a higher temperature is not due to the vibrations becoming so violent as to set the molecules spinning, when, of course, their polarity would be of no avail to produce magnetisation. We know, at all events, that when the change from the magnetic to the non-magnetic state occurs, there is a profound molecular change,

and heat is absorbed which is given out again when the reverse change takes place. In cooling from a red heat, the iron actually extends at the moment when this change takes place (as was shown by Gore), and so much heat is given out that (as Barrett observed) it reglows, becoming brightly red, though just before the change it had cooled so far as to be quite dull. [Experiment, exhibiting retraction and reglow in cooling, shown by means of a long steel wire, heated to redness by the electric current.] The changes which occur in iron and steel about the temperature of redness are very complex, and I refer to this as only one possible direction in which a key to them may be sought.

An interesting illustration of the use of these models has reached me, only this morning, from New York. In a paper just published in the *Electrical World*,\* Mr. Arthur Hoopes supports the theory I have laid before you by giving curves which show the connection, determined experimentally, between the resultant polarity of a group of little pivoted magnets and the strength of the magnetic field, when the field is applied, removed, reversed, and so on. I shall throw these curves upon the screen, and, rough as they are, in consequence of the limited number of the magnets, you see that they succeed remarkably well in reproducing the features which we know the curves for solid iron to possess.

It may, perhaps, be fairly claimed that the models whose behaviour we have been considering have a wider application in physics than merely to elucidate magnetic processes. The molecules of bodies may have polarity which is not magnetic at all—polarity, for instance due to static electrification—under which they group themselves in stable forms, so that energy is dissipated whenever these are broken up and rearranged. When we strain a solid body beyond its limit of elasticity, we expend work irrecoverably in overcoming, as it were, internal friction. What is this internal friction due to but the breaking and making of molecular ties? And if internal friction is to be ascribed to that, why not also the surface friction which causes work to be spent when one body rubs upon another? In a highly suggestive passage of one of his writings, † Clerk Maxwell threw out the hint that many of the irreversible processes of physics are due to the breaking up and reconstruction of molecular groups. These models help us to realise Maxwell's notion, and, in studying them to-night, I think we may claim to have been going a step or two forward where that great leader pointed the way. [J. A. E.]

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\* Reprinted in the *Electrician* of May 29th, 1891.

† 'Encyc. Brit.' Art. "Constitution of Bodies."

Friday, June 12, 1891.

SIR FREDERICK BRAMWELL, Bart. D.C.L. F.R.S. Honorary Secretary  
and Vice-President, in the Chair.

HAROLD B. DIXON, Esq. M.A. F.R.S. Professor of Chemistry in the  
Owens College, Manchester.

*The Rate of Explosions in Gases.*

THE rapid act of chemical change, which follows the kindling of an explosive mixture of gases, has of late years attracted the interest both of practical engineers and of theoretical chemists. To utilise for motive power the expansive force of ignited gases; to minimise the chance of disastrous conflagrations of fire-damp in coal mines; to follow the progress of chemical changes under the simplest conditions, are some among the problems presented to us in industry or science, demanding for their solution a knowledge of the phenomena of the explosions of gases.

To understand the nature of explosions in gases it is necessary to know certain fundamental properties of the explosive mixture. With this object in view experimenters have sought to determine for various mixture of gases:—the heat of chemical combination; the temperature of inflammation; the pressure developed; and lastly, the rate at which the explosion is propagated under different conditions.

It is on the last of these problems—the determination of the velocity with which the flame travels through the gas—that I have been asked to speak.

Twenty-four years ago Bunsen described a method of measuring the rapidity of the flame in gas explosions. Passing a mixture of explosive gases through an orifice at the end of a tube and igniting the gases as they issued into the air, he determined the rate at which the gases must be driven through the tube to prevent the flame passing back through the opening, and exploding inside the tube. By this method he found that the rate of propagation of the ignition of hydrogen and oxygen was 34 metres per second, while the rate

of ignition of carbonic oxide and oxygen was less than 1 metre per second. Bunsen applied these results to the rate of explosion of gases in closed vessels, and his results were accepted without cavil for fourteen years.

By 1880 facts began to accumulate which seemed inconsistent with Bunsen's conclusions. For instance, between 1876-80 I had several times observed that the flame of carbonic oxide and oxygen travelled in a long eudiometer too quickly to be followed by the eye. Mr. A. V. Harcourt, in his investigation of an explosion which happened in a large gas main near the Tottenham Court Road in 1880, was led to the conclusion that the flame travelled at a rate exceeding 100 yards per second. In the winter of 1880-1 I noticed the rapid increase of velocity as a flame of carbon bisulphide with nitric oxide travelled down a long glass vessel; and shortly afterwards I attempted to measure the rate of explosion of carbonic oxide and oxygen by photographing on a moving plate the flash at the beginning and end of a long tube. The two flashes appeared to be simultaneous to the eye, but no record of the rate was obtained, for the apparatus was broken to pieces by the violence of the explosion.

In July 1881 two papers appeared in the 'Comptes Rendus,' one by M. Berthelot, the other by MM. Mallard and Le Chatelier. Both papers announced the discovery of the enormous velocity of explosion of gaseous mixtures. Other papers quickly followed by the same authors. M. Berthelot made the important discovery that the rate of explosion rapidly increases from its point of origin until it reaches a maximum which remains constant, however long the column of gases may be. This maximum M. Berthelot states to be independent of the pressure of the gases, of the material of the tube, and of its diameter above a small limit. The rate of explosion thus forms a new physico-chemical constant, having important theoretical and practical bearings. The name "L'Onde Explosive" is given by Berthelot to the flame when propagated through an explosive mixture of gases at the maximum velocity.

While Berthelot, associated with Vieille, was measuring the rate of the "explosion-wave" for various mixtures of gases, Mallard and Le Chatelier continued the study of the preliminary phenomena of explosion which precede the formation of the "wave." They showed by photographing on a revolving cylinder:—(1) that when a mixture such as nitric oxide and carbon bisulphide is ignited at the open end of a tube, the flame travels a certain distance (depending on the diameter and length of the tube) at a uniform velocity; (2) that at a certain point in the tube, vibrations are set up which alter the character of the flame, and that these vibrations become more intense, the flame swinging backwards and forwards, with oscillations of increasing amplitude; and (3) that the flame either goes out altogether, or that the rest of the gas detonates with extreme velocity. Again, when a mixture of gases was fired near the closed end of the

tube, they found the velocity of the flame regularly increased, as far as their instruments were able to record the rapidly increasing pace.

Mixtures of coal-gas with air, and of fire-damp with air, show phenomena of the first and second kind. Ignited at the open end of a tube these mixtures burn at a uniform rate for a certain distance, and then the flame begins to vibrate.

The vibrations acquire greater or less velocity according to the nature of the mixture and the conditions of the experiment, but the third régime of uniform maximum velocity is not set up. In narrow tubes the explosion soon dies out.

The phenomena studied by Mallard and Le Chatelier have been observed on a large scale in explosions in coal mines. It has been noticed that little damage was caused at the source of an explosion, and for a distance varying from 50 to 80 yards from the origin of the flame, while beyond that distance falls of roof, broken tubs, and blown-out stoppings have testified to the violence exerted by the explosion. Great as the destruction is which an explosion of fire-damp and air causes in a mine, it is fortunate that these mixtures do not *detonate*.

Passing on to Berthelot's researches on the régime of detonation, I will briefly summarise the results he has arrived at.

The actual velocities of explosion are compared by Berthelot with the mean velocity of translation of the gaseous products of combustion, supposing these products to contain all the heat that is developed in the reaction.

For instance, we know the total heat given out when hydrogen and oxygen combine. If this heat is contained in the steam produced, we can calculate what its temperature must be if we know its heat capacity. And if we know the temperature of the steam, we can calculate the mean velocity with which the molecules must be moving. Now Berthelot supposes that the heat is all contained in the steam produced. He assumes that the heat capacity of steam is the same as the sum of those of its constituents; and he supposes, moreover, that the steam is heated at constant pressure. Making these assumptions, he calculates out the theoretical mean velocity of the products of combustion of various mixtures, and finds a close accordance between these numbers and the explosion rates of the same mixtures. He concludes that the explosive wave is propagated by the impact of the products of combustion of one layer upon the unburnt gases in the next layer, and so on to the end of the tube at the rate of movement of the products of combustion themselves. If his theory is true, it accounts not only for the extreme rapidity of explosion of gaseous mixtures, and gives us the means of calculating the maximum velocity obtainable with any mixture of gases, but it also affords us information on the specific heats of gases at very high temperatures, and it explains the phenomena of detonation whether of gases or of solid or liquid explosives.



Table I. shows the explosion rates found by Berthelot, compared with the theoretical velocity of the products of combustion :—

TABLE I.  
BERTHELOT'S EXPERIMENTS.

Gaseous Mixture.	Velocity In Metres per Second.	
	Theoretical.	Found.
$\text{H}_2 + \text{O}$ Hydrogen and oxygen.	2830	2810
$\text{H}_2 + \text{N}_2\text{O}$ Hydrogen and nitrous oxide.	2250	2284
$\text{CO} + \text{O}$ Carbonic oxide and oxygen.	1940	1090
$\text{CO} + \text{N}_2\text{O}$ Carbonic oxide and nitrous oxide.	1897	1106
$\text{CH}_4 + \text{O}_4$ Marsh gas and oxygen.	2427	2287
$\text{C}_2\text{H}_4 + \text{O}_4$ Ethylene and oxygen.	2517	2210
$\text{C}_2\text{N}_2 + \text{O}_4$ Cyanogen and oxygen.	2190	2195
$\text{C}_2\text{H}_2 + \text{O}_3$ Acetylene and oxygen.	2660	2482
$\text{CO} + \text{H}_2 + \text{O}_2$ Carbonic oxide, hydrogen, and oxygen.	2236	2908

Two points in Table I. favoured the view that Berthelot might have here given the true theory of explosions: first, the close coincidence between the rates of explosion of hydrogen both with oxygen and nitrous oxide with the calculated mean velocities of the products of combustion; and secondly, the great discordance between the found and calculated rates for carbonic oxide with both oxygen and nitrous oxide. I had previously discovered that pure carbonic oxide cannot be exploded either with pure oxygen or pure nitrous oxide. The discordance found by Berthelot was what I should have expected from my own experiments.

A consideration of Berthelot's results, published in full in the 'Annales de Chimie,' led me to think it would be useful to repeat and extend these experiments. My objects were chiefly: (1) to determine as accurately as possible the rate of the explosion-wave for

some well-known mixtures ; (2) to measure the rate of the explosion wave in carbonic oxide with different quantities of steam ; and (3) to determine the influence of inert gases on the propagation of the wave.

1. The results obtained with hydrogen and oxygen, with hydrogen and nitrous oxide, and with marsh gas and oxygen in exact proportions for complete combustion, were in close accordance with the mean results of Berthelot ; for ethylene, acetylene, and cyanogen my numbers differed appreciably, but in no case differed by more than 7 per cent. from the rates observed by Berthelot :—

TABLE II.  
VELOCITY OF EXPLOSION IN METRES PER SECOND.

		Berthelot	Dixon.
Hydrogen and oxygen	$H_2 + O$	2810	2821
Hydrogen and nitrous oxide	$H_2 + N_2 O$	2284	2305
Marsh gas and oxygen	$CH_4 + O_2$	2287	2322
Ethylene and oxygen	$C_2 H_4 + O_2$	2210	2364
Acetylene and oxygen	$C_2 H_2 + O_2$	2482	2391
Cyanogen and oxygen	$C_2 N_2 + O_2$	2195	2321

The general agreement between these measurements left no room for doubt about the substantial accuracy of Berthelot's experiments. The formula he gives does, therefore, express with a close degree of approximation the rates of explosion of many gaseous mixtures.

2. The formula fails for the explosion of carbonic oxide with oxygen or nitrous oxide. This was to be expected if, in the detonation of carbonic oxide in a long tube, the oxidation is effected indirectly by means of steam, as it is in the ordinary combustion of the gas. Measurements of the rate of explosion of carbonic oxide and oxygen in a long tube showed that the rate increased as steam was added to the dry mixture, until a maximum velocity was attained when between 5 and 6 per cent. of steam was present.

3. When electrolytic gas was mixed with an excess of either hydrogen or oxygen the rate of explosion was found to be altered ; the addition of hydrogen increasing the velocity, the addition of oxygen diminishing it. The addition of an inert gas nitrogen, incapable of taking part in the chemical change, produced the same effect as the addition of oxygen—one of the reacting substances—only the retarding effect of nitrogen was less marked than that of an equal volume of oxygen. The retardation of the explosion-wave

caused by the addition of an inert gas to electrolytic gas evidently, therefore, depends upon the volume and the density of the gas added. In the following table the retarding effect of oxygen and nitrogen, on the explosion of electrolytic gas, is compared:—

TABLE III.

RATE OF EXPLOSION OF ELECTROLYTIC GAS WITH EXCESS OF OXYGEN AND NITROGEN.

Volume of oxygen added to $H_2 + O$ Rate. }	$O_1$ 2328	$O_2$ 1927	$O_3$ 1690	$O_4$ 1281
Volume of nitrogen added to $H_2 + O$ Rate. }	$N_1$ 2426	$N_2$ 2055	$N_3$ 1822	$N_4$ —

I think it a fair inference from these facts to conclude, when the addition of a gas to an explosive mixture retards the rate of explosion by an amount proportional to its volume and density, that such added gas is inert as far as the propagation of the wave is concerned, and that any change which it may undergo takes place after the wave-front has passed by—in other words, is a *secondary* change.

This principle has been applied to determine whether, in the combustion of gaseous carbon, the oxidation to carbonic acid is effected in one or two stages—an important question, on which there is little experimental evidence. If, for instance, in the combustion of a hydrocarbon, or of cyanogen, the carbon is first burnt to carbonic oxide, which subsequently is burnt to carbonic acid, the rate of the explosion-wave should correspond with the carbonic oxide reaction, in this case the primary reaction; whereas, if the carbon of these gases burns to carbonic acid directly, in one stage, then the rate of the explosion-wave should correspond with the complete reaction.

Now, if we adopt Berthelot's formula as a working hypothesis, we can calculate the theoretical rates of explosion of marsh gas, ethylene, or cyanogen: (1) on the supposition that the carbon burns directly to  $CO_2$ , and (2) on the supposition that the carbon burns first to  $CO$ , and the further oxidation is a subsequent or secondary reaction. On the first supposition, if 100 represents the rate of explosion of these three gases burning to carbonic oxide, the addition of the oxygen required to burn the gases to carbonic acid should *increase* the rate of explosion:—

	Marsh Gas.	Ethylene.	Cyanogen.
Calculated rate of explosion when burnt to $CO_2$ .. }	104	103	107

Whereas if these gases really burn first to carbonic oxide, and the

extra oxygen is inert in propagating the explosion-wave, then the addition of this inert oxygen would diminish the rate of explosion:—

	Marsh Gas.	Ethylene.	Cyanogen.
Calculated rate of explosion when burnt to CO with inert oxygen present .. }	92	88	87

The experiments show that if 100 be taken as the rate of explosion when the oxygen is only sufficient to burn the carbon to carbonic oxide, the following are the rates found when oxygen is added sufficient to burn the carbon to carbonic acid:—

	Marsh Gas	Ethylene.	Cyanogen.
Rates found .. .. .	94	92	84

The results are, therefore, in favour of the view that, in the explosion of these gases, the carbon is first burnt to carbonic oxide.

But stronger evidence on this point is obtained by comparing the explosion rate of these gases (1) when fired with oxygen sufficient to burn the carbon in them to carbonic acid, and (2) when nitrogen is substituted for the oxygen in excess of that required to burn the carbon to carbonic oxide. We have seen that oxygen added to electrolytic gas hinders the explosion more than nitrogen. In precisely the same way oxygen added to a mixture of equal volumes of cyanogen and oxygen hinders the explosion more than the same volume of nitrogen. The conclusion we must come to is that the oxygen added to the mixture expressed by the formula  $C_2N_2 + O_2$  is as inert (so far as the propagation of the explosion-wave is concerned) as oxygen added to the mixture expressed by the formula  $H_2 + O$ . The same phenomena occur in the explosion of marsh gas, ethylene, and acetylene. In all these cases the substitution of nitrogen for the oxygen required to burn the carbon from carbonic oxide to carbonic acid *increases* the velocity of the explosion. These facts seem only consistent with the view that the carbon burns directly to carbonic oxide, and the formation of carbonic acid is an after-occurrence.

Finally, the rates of explosion of cyanogen and the hydrocarbons, when their carbon is burnt to carbonic oxide, have been found greater than the velocities calculated from Berthelot's formula. This accords with the observation previously made that the rate of explosion of electrolytic gas with excess either of hydrogen or oxygen is far higher than the calculated rate. It would seem probable that the theoretical rates as calculated by Berthelot should be modified, in spite of the close agreement which his numbers show. I think the low rates found, when hydrogen, marsh gas, cyanogen, &c., are exploded with equivalent proportions of oxygen, depend partly on the carbon burning to carbonic oxide and partly on the dissociation of the steam at the high temperature. If the formula is modified in these respects, velocities can be calculated which agree with the experimental results where dissociation does not occur.

I suggest the following modifications: (1) the specific heats should be taken at constant volume instead of at constant pressure; (2) the density of the gas should be taken as the mean of the burnt and unburnt molecules, instead of that of the burnt molecules alone; and (3) a correction should be made for the alteration of volume by the chemical reaction, which in some cases increases, in others diminishes, the volume.

The rates so calculated agree with the explosion rates of cyanogen when burnt to carbonic oxide either by oxygen, nitrous oxide, or nitric oxide; with the explosion rates of hydrogen and oxygen with a large excess either of hydrogen, oxygen, or nitrogen; with the explosion rates of ethylene and acetylene with oxygen and a large excess of nitrogen; and, lastly, with the explosion rates of hydrogen and chlorine with an excess of hydrogen.

In conclusion, I would say that these experiments have amply confirmed the truth of Berthelot's statement that the explosion-wave is a "specific constant" for every gaseous mixture; that it has been shown that the rate of explosion depends upon the primary reaction occurring, and that the determination of the rate may throw some light on what is now so obscure—the mode in which chemical changes are brought about; and, finally, that it does not seem impossible that a connection between the rate of the molecules and the rate of the explosion may be worked out, which will give us some definite information on points of high interest in the theory of gases.

[H. B. D.]

## THE FARADAY CENTENARY.\*

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Wednesday, June 17, 1891.

H.R.H. THE PRINCE OF WALES, K.G. F.R.S. Vice-Patron,  
in the Chair.

**THERE** were also present—The Duke of Northumberland (President), Lord Morris, Sir William Thomson (Pres. R.S.), Sir George Stokes, M.P. Sir William Grove, Count Tornielli (the Italian Ambassador), Sir Frederick Leighton, Sir James Crichton Browne, Sir Joseph Lister, Sir Frederick Abel, Sir William Bowman, Sir Archibald Geikie, Sir Henry Roscoe, M.P. Sir Somers Vine, Sir Frederick Bramwell, Professor Dewar, and Professor Horsley.

**THE PRINCE OF WALES** opened the proceedings with the following address:—

Ladies and Gentlemen,—I can well remember that two-and-twenty years ago I had the high privilege of presiding at a meeting here. That meeting was a very large one, and included many of the most eminent scientific men of the day. Among those present on that occasion, I remember, were the illustrious chemist, Professor Dumas, Sir Edward Sabine, Sir Roderick Murchison, Sir Henry Holland, a very old personal friend of mine, Dr. Bence Jones, Mr. Warren de la Rue, and many others, who I regret to say have now passed away. The object of our meeting on that occasion was to select a suitable memorial to the memory of the great Faraday, the eminent chemist and philosopher, who, I may say, was also the founder of modern electricity. As you are well aware, the fine statue by Foley, which is in the hall below, was, we thought, a suitable memorial to that great man. As for myself personally, I feel proud to think that in the days of my boyhood my brother and myself used to attend his chemical lectures here about Christmas time, and I shall ever remember the admirable and lucid way in which he delivered those lectures to us who were mere boys, and gave us a deep interest in chemistry, which we kept up for many years, and which I had the opportunity of practising at the University of Oxford. I can only regret that I have not since had the time to pursue that interesting science. To-day is a memorable day, for this year we celebrate the centenary of the birth of that great man; and we all of us have reason to feel grateful that two such eminent men as Lord Rayleigh and Professor Dewar should have consented to give lectures on the work of the great Faraday. I have only now to beg Lord Rayleigh to give us his address.

**LORD RAYLEIGH** said that the man whose name and work they were

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\* Michael Faraday, born 22nd September, 1791.

celebrating was identified in a remarkable degree with the history of this Institution. If they could not take credit for his birth, in other respects they could hardly claim too much. During a connection of fifty-four years, Faraday found there his opportunity, and for a large part of the time his home. The simple story of his life must be known to most who heard him. Fired by contact with the genius of Davy, he volunteered his services in the laboratory of the Institution. Davy, struck with the enthusiasm of the youth, gave him the desired opportunity, and, as had been said, secured in Faraday not the least of his discoveries. The early promise was indeed amply fulfilled, and for a long period of years by his discoveries in chemistry and electricity Faraday maintained the renown of the Royal Institution and the honour of England in the eye of the civilised world. He should not attempt in the time at his disposal to trace in any detail the steps of that wonderful career. The task had already been performed by able hands. In their own 'Proceedings' they had a vivid sketch from the pen of one whose absence that day was a matter of lively regret. Dr. Tyndall was a personal friend, had seen Faraday at work, had enjoyed opportunities of watching the action of his mind in face of a new idea. All that he could aim at was to recall, in a fragmentary manner, some of Faraday's great achievements, and, if possible, to estimate the position they held in contemporary science.

Whether they had regard to fundamental scientific import, or to practical results, the first place must undoubtedly be assigned to the great discovery of the induction of electrical currents. He proposed first to show the experiment in something like its original form, and then to pass on to some variations, with illustrations from the behaviour of a model, whose mechanical properties were analogous. He was afraid that these elementary experiments would tax the patience of many who heard him, but it was one of the difficulties of his task that Faraday's discoveries were so fundamental as to have become familiar to all serious students of physics.

The first experiment required them to establish in one coil of copper wire an electric current by completing the communication with a suitable battery; that was called the primary circuit, and Faraday's discovery was this: That at the moment of the starting or stopping of the primary current in a neighbouring circuit, in the ordinary sense of the words completely detached, there was a tendency to induce a current. He had said that those two circuits were perfectly distinct, and they were distinct in the sense that there was no conducting communication between them, but, of course, the importance of the experiment resided in this—that it proved that in some sense the circuits were not distinct; that an electric current circulating in one does produce an effect in the other, an effect which is propagated across a perfectly blank space occupied by air, and which might equally well have been occupied by vacuum. It might appear that that was a very simple and easy experiment, and of course it was so in a modern laboratory, but it was otherwise at

the time when Faraday first made it. With all his skill, Faraday did not light upon the truth without delay and difficulty. One of Faraday's biographers thus wrote:—"In December 1824, he had attempted to obtain an electric current by means of a magnet, and on three occasions he had made elaborate and unsuccessful attempts to produce a current in one wire by means of a current in another wire, or by a magnet. He still persevered, and on August 29, 1831—that is to say, nearly seven years after his first attempts—he obtained the first evidence that an electric current induced another in a different circuit." On September 23rd, he writes to a friend, R. Phillips: "I am busy just now again with electro-magnetism, and think I have got hold of a good thing, but cannot say; it may be a weed instead



of a fish that, after all my labour, I at last haul up." We now know that it was a very big fish indeed. Lord Rayleigh proceeded to say that he now proposed to illustrate the mechanics of the question of the induced current by means of a model (see figure), the first idea of which was due to Maxwell. The one actually employed was a combination known as Huygens's gear, invented by him in connection with the winding of clocks. Two similar pulleys, A B, turn upon a piece of round steel fixed horizontally. Over those is hung an endless cord, and the two bights carry similar pendant pulleys, C, D, from which again hang weights, E, F. The weight of the cord being negligible, the system is devoid of potential energy; that is, it will balance, whatever may be the vertical distance between C and D. Since either pulley, A, B, may turn independently of the other, the system is capable of two independent motions. If A, B turn in the same direction and with the same velocity one of the pendant pulleys, C, D rises, and the other falls. If, on the other hand, the motions of A, B are equal and opposite, the axes of the pendant pulleys and the attached weights remain at rest. In the electrical analogue the rotatory velocity of A corresponds to a current in a primary circuit, that

of B to a current in a secondary. If, when all is at rest, the rotation of A be suddenly started, by force applied at the handle or otherwise, the inertia of the masses E, F opposes their sudden movement, and the consequence is that the pulley B turns *backwards*, i. e. in the opposite direction to the rotation imposed upon A. This is the current induced in a secondary circuit when an electromotive-force begins to act in the primary. In like manner, if A, having been for some time in uniform movement, suddenly stops, B enters into motion in the direction of the former movement of A. This is



the secondary current on the break of the current in the primary circuit. It might perhaps be supposed by some that the model was a kind of trick. Nothing could be further from the truth. The analogy of the two things was absolutely essential. So far was this the case that precisely the same argument and precisely the same mathematical equations proved that the model and the electric currents behaved in the way in which they had seen them behave in the experiment. That might be considered to be a considerable triumph of the modern dynamical method of including under the same head phenomena the details of which might be so different as in this case. If they had a current which alternately stopped and started, and so on, for any length of time, they, as it were, produced in a permanent manner some of the phenomena of electrical induction; and if it were done with sufficient rapidity it would be evident that something would be going on in the primary and in the secondary circuit. The particular apparatus by which he proposed to illustrate those effects of the alternating current was devised by a skilful American electrician, Prof. Elihu Thomson, and he had no doubt it would be new to many. The alternating current was led into the electro-magnet by a suitable lead; if another electric circuit, to be called the secondary circuit, was held in the neighbourhood of that, currents would be induced and might be made manifest by suitable means. Such a secondary circuit he held in his hand, and it was connected with a small electric glow-lamp. If a current of sufficient intensity were induced in that secondary circuit it would pass through the lamp, which would be rendered incandescent. [Illustrating.] It was perfectly clear there was no con-juring there; the incandescent lamp brightened up. One of the first questions which presented itself was, what would be the effect of putting something between? Experimenting with a glass plate, he showed there was no effect, but when they tried a copper plate the lamp went completely out, showing that the copper plate was an absolute screen to the effect, whatever it might be. Experiments of that kind, of course in a much less developed and striking form, were made by Faraday himself, and must be reckoned amongst some of his greatest discoveries.

Before going further, he might remark on what strong evidence they got in that way of the fact that the propagation of the electric energy which, having its source in the dynamo downstairs, eventually illuminated that little lamp, was not merely along the wires, but was capable of bridging over and passing across a space free from all conducting material, and which might be air, glass, or, equally well, vacuum. Another kindred effect of a striking nature, devised by Prof. Elihu Thomson, consisted in the repulsive action which occurred between the primary current circulating around a magnet and the current induced in a single hoop of aluminium wire. Illustrating this by experiment, he showed that the repulsion was so strong as to throw the wire up a considerable height. Those effects were com-

monly described as dependent upon the mutual induction between two distinct circuits, one being that primarily excited by a battery or other source of electricity, while the other occurred in a detached circuit. Many surprising effects, however, depended on the reactions which took place at different parts of the same circuit. One of these he illustrated by the decomposition of water under the influence of self-induction.

About the time the experiments of which he had been speaking were made, Faraday evidently felt uneasiness as to the soundness of the views about electricity held by his contemporaries, and to some extent shared by himself, and he made elaborate experiments to remove all doubt from his mind. He re-proved the complete identity of the electricity of lightning and of the electricity of the voltaic cell. He evidently was in terror of being misled by words which might convey a meaning beyond what facts justified. Much use was made of the term "poles" of the galvanic battery. Faraday was afraid of the meaning which might be attached to the word "pole," and he introduced a term since generally substituted, "electrode," which meant nothing more than the way or path by which the electricity was led in. "Electric fluid" was a term which Faraday considered dangerous, as meaning more than they really knew about the nature of electricity, and as was remarked by Maxwell, Faraday succeeded in banishing the term "electric fluid" to the region of newspaper paragraphs.

Diamagnetism was a subject upon which Faraday worked, but it would take him too long to go into that subject, though he must say a word or two. Faraday found that whereas a ball of iron or nickel or cobalt, when placed near a magnet or combination of magnets, would be attracted to the place where the magnetic force was the greatest, the contrary occurred if for the iron was substituted a corresponding mass of bismuth or of many other substances. The experiments in diamagnetism were of a microscopic character, but he would like to illustrate one position of Faraday's, developed years afterwards by Sir Wm. Thomson, and illustrated by him in many beautiful experiments, only one of which he now proposed to bring before them. Supposing they had two magnetic poles, a north pole and a south pole, with an iron ball between them, free to move along a line perpendicular to that joining the poles, then, according to the rule he had stated, the iron ball would seek an intermediate position, the place at which the magnetic force was the greatest. Consequently, if the iron ball be given such a position, they would find it tended with considerable force to a central position of equilibrium; but if, instead of using opposite poles, they used two north poles, they would find that the iron ball did not tend to the central position, because that was not the place in which the magnetic force was the greatest. At that place there was no magnetic force, for the one pole completely neutralised the action of the other. The greatest force would be a little way out, and that, according to Faraday's observa-

tions, systematised and expressed in the form of mathematical law by Sir Wm. Thomson, was where the ball would go. [This was illustrated by experiment.]

The next discovery of Faraday to which he proposed to call attention was one of immense significance from a scientific point of view, the consequences of which were not even yet fully understood or developed. He referred to the magnetisation of a ray of light, or what was called in more usual parlance the rotation of the plane of polarisation under the action of magnetic force. It would be hopeless to attempt to explain all the preliminaries of the experiment to those who had not given some attention to those subjects before, and he could only attempt it in general terms. It would be known to most of them that the vibrations which constituted light were executed in a direction perpendicular to that of the ray of light. By experiment he showed that the polarisation which was suitable to pass the first obstacle was not suitable to pass the second, but if by means of any mechanism they were able after the light had passed the first obstacle, to turn round the vibration, they would then give it an opportunity of passing the second obstacle. That was what was involved in Faraday's discovery. [Experiment.] As he had said, the full significance of the experiment was not yet realised. A large step towards realising it, however, was contained in the observation of Sir Wm. Thomson, that the rotation of the plane of polarisation proved that something in the nature of rotation must be going on within the medium when subjected to the magnetising force, but the precise nature of the rotation was a matter for further speculation, and perhaps might not be known for some time to come.

When first considering what to bring before them he thought, perhaps, he might include some of Faraday's acoustical experiments, which were of great interest, though they did not attract so much attention as his fundamental electrical discoveries. He would only allude to one point which, as far as he knew, had never been noticed, but which Faraday recorded in his acoustical papers. "If during a strong steady wind, a smooth flat sandy shore, with enough water on it, either from the receding tide or from the shingle above, to cover it thoroughly, but not to form waves, be observed in a place where the wind is not broken by pits or stones, stationary undulations will be seen over the whole of the wet surface. . . . These are not waves of the ordinary kind, they are (and this is the remarkable point) accurately parallel to the course of the wind." When he first read that statement, many years ago, he was a little doubtful as to whether to accept the apparent meaning of Faraday's words. He knew of no suggestion of an explanation of the possibility of waves of that kind being generated under the action of the wind, and it was, therefore, with some curiosity that two or three years ago, at a French watering-place, he went out at low tide, on a suitable day when there was a good breeze blowing, to see if he could observe anything of

the waves described by Faraday. For some time he failed absolutely to observe the phenomenon, but after a while he was perfectly well able to recognise it. He mentioned that as an example of Faraday's extraordinary powers of observation, and even now he doubted whether anybody but himself and Faraday had ever seen that phenomenon.

Many matters of minor theoretic interest were dealt with by Faraday, and reprinted by him in his collected works. He was reminded of one the other day by a lamentable accident which occurred owing to the breaking of a paraffin lamp. Faraday called attention to the fact, though he did not suppose he was the first to notice it, that by a preliminary preparation of the lungs by a number of deep inspirations and expirations, it was possible so to aerate the blood as to allow of holding the breath for a much longer period than without such a preparation would be possible. He remembered some years ago trying the experiment, and running up from the drawing-room to the nursery of a large house without drawing any breath. That was obviously of immense importance, as Faraday pointed out, in the case of danger from suffocation by fire, and he thought that possibly the accident to which he alluded might have been spared had the knowledge of the fact to which Faraday drew attention been more generally diffused.

The question had often been discussed as to what would have been the effect upon Faraday's career of discovery had he been subjected in early life to mathematical training. The first thing that occurred to him about that, after reading Faraday's works, was that one would not wish him to be anything different from what he was. If the question must be discussed, he supposed they would have to admit that he would have been saved much wasted labour, and would have been better *en rapport* with his scientific contemporaries if he had had elementary mathematical instruction. But mathematical training and mathematical capacity were two different things, and it did not at all follow that Faraday had not a mathematical mind. Indeed, some of the highest authorities had held (and there could be no higher authority on the subject than Maxwell) that his mind was essentially mathematical in its qualities, although they must admit it was not developed in a mathematical direction. With these words of Maxwell he would conclude:—"The way in which Faraday made use of his idea of lines of force in co ordinating the phenomena of electric induction shows him to have been a mathematician of high order, and one from whom the mathematicians of the future may derive valuable and fertile methods."

## THE FARADAY CENTENARY.

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Friday, June 26, 1891.

The DUKE OF NORTHUMBERLAND, K.G. D.C.L. LL.D. President,  
in the Chair.

THERE were also present—Lord Halsbury, Sir Lyon Playfair, Sir Richard Webster, Sir Edward Fry, Sir William Thomson, Sir Joseph Lister, Sir James Crichton Browne, Lord Rayleigh, Sir Joseph Fayrer, Sir William Bowman, Sir Frederick Abel, Dr. Frankland, Professor Odling, Mr. Ludwig Mond, and Sir Frederick Bramwell.

*The Chemical Work of Faraday in relation to Modern Science.*

By PROFESSOR DEWAR, M.A. F.R.S.

Prof. Dewar commenced his lecture by saying that his eminent colleague had done such ample justice to the physical side of Faraday's work, that his own task would be limited to dealing with those early researches in which he developed that astounding manipulative power which enabled him to conduct his subsequent electrical investigations in so remarkable a manner. He proposed to give a brief sketch of the more important of the distinctive chemical labours of Faraday, and then to select one of the many veins of investigation he had opened up, and show what had resulted from its development.

Faraday's chemical work might be divided into the following groups or periods:—Period of Analytic Work. Organic Research. Study of Gaseous Properties. Investigations on Steel and Glass. Determination of Electro-chemical Equivalents. Regulation. Action of Metals on Light. Work on Chemical Manipulation. Published Lectures.

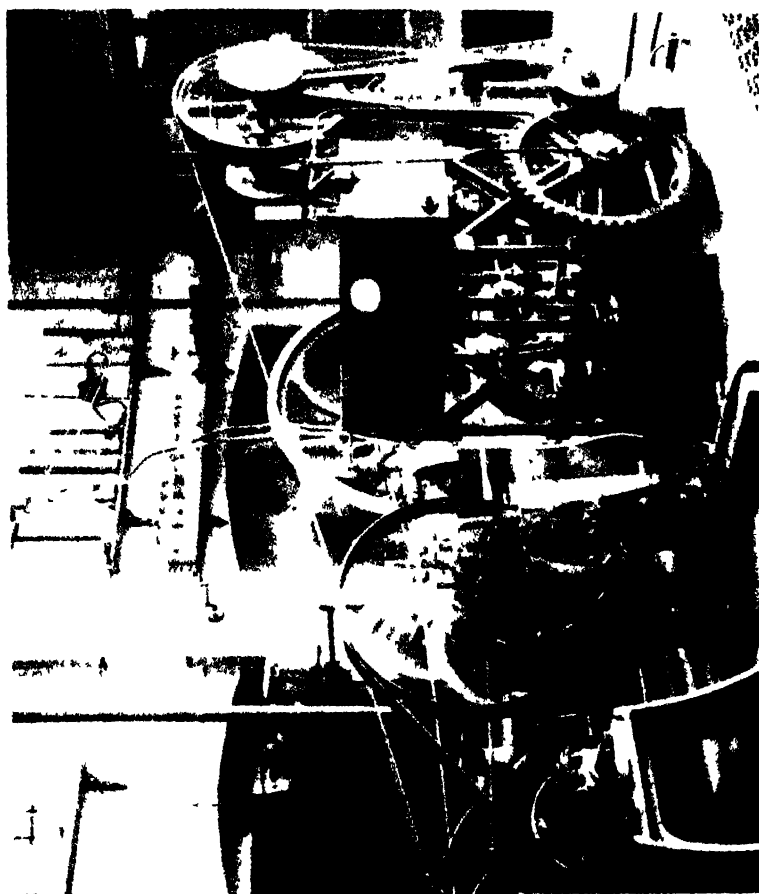
Having given a short résumé of Faraday's progress through these subjects, Prof. Dewar referred to his first great work in organic research, the production of two compounds of chlorine and carbon, the perchloride and the protochloride, and the determination of the composition of "Julian's chloride of carbon." The original specimens prepared by Faraday were exhibited, and it was pointed out that the discoverer's analyses of these bodies were absolutely accurate, notwithstanding the difficulties attending such work at that time. His discovery of "bicarburet of hydrogen" (now widely known and largely manufactured as benzol), and a "new hydrocarbon" (now known as butylene) was then described, it being pointed out that having regard to the methods of working which Faraday had to employ, the isolation and determination of the composition of such bodies was marvellous, and was to be explained only by his wonderful manipulative skill.

Probably Faraday's most remarkable discovery in organic chemistry was the fact that naphthalene could be dissolved by strong sulphuric acid, and that when thus dissolved the solution did not precipitate naphthalene on being treated with water. That enabled him to prove combination between sulphuric acid and a hydrocarbon. The body, which he called "sulpho-naphthalic acid," is probably the first of the sulpho-acids now so largely employed in the colour industry.

Faraday's next important work was an investigation into the properties of combinations of steel with other metals, in the course of which he demonstrated the now well-recognised fact that an admixture of such minute proportions as one-five-hundredth of such metals as silver, nickel, palladium, &c., will entirely alter the character of the metal. Concurrently with this, he worked on the improvement of optical glass; and it was observed that although the fruits of his labours in this direction lay dormant for some time, they ultimately resulted in one of his most important discoveries, namely, the rotation of the plane of polarisation in the magnetic field. The glass produced by Faraday by the fusion of oxide of lead with boracic acid was selected by him because of its superior fluidity combined with great density. (Experiments were given illustrating the peculiar physical and electrical properties of the Faraday glass.)

The next research was that on the liquefaction of gases, which, although carried out by Faraday, was nevertheless done at the instigation of Davy. Davy had discovered a substance which proved to be a hydrate of chlorine, and which he found could be kept either in ice or in sealed tubes. Faraday had produced a quantity of this substance during the cold weather, and had made an analysis of it. Davy then suggested that it should be heated in a sealed tube, and, without saying what he really expected to take place, indicated that one of three things would happen, namely, that it would either melt, act on water, or produce liquid chlorine. The latter event happened, and opened up vast possibilities, the prosecution of which Davy left to Faraday. (Experiment on the liquefaction of chlorine given.) The necessity of obtaining tubes strong enough to stand the pressure required for the liquefaction experiments led Faraday to make investigations at this time into the production of bottle and other glass.

Faraday next turned his attention to researches on the electro-chemical relations of bodies, crystallisation, and the action of metals on light. It was in connection with the research on crystallisation in 1856 that Faraday made his interesting discovery of the phenomenon of regelation, by virtue of which two portions of a piece of ice, after being severed, freeze together again on being brought into contact, even when the temperature of the surrounding medium is higher than the freezing point of water. Although discovered by Faraday, it was not until comparatively recent times that the explanation of the phenomenon was given, and its influence on glacial motion clearly established. (Experiment on regelation shown.)



Specimens, arranged and tabulated by himself, of Faraday's last research on the optical properties of gold leaf in a highly attenuated form were exhibited and described.

Turning then to the special subject of the evening's discourse, the liquefaction of gases, Prof. Dowar stated that although Faraday made his first researches in this direction as early as 1823, the matter lay dormant for many years, until his interest in it was reawakened by Thilorier's discovery that solid carbonic acid could be produced in the form of a snow-like substance, boiling at  $-80^{\circ}\text{C}.$ , and capable of being handled. Faraday was the first to introduce this discovery into England in a lecture given at the Royal Institution on the 18th May, 1838; and, thereafter, by its aid, he resumed his work on the liquefaction of the various gases which had resisted his former efforts. All through the summer of 1844 he was busily employed at this work, using the low temperatures, which Thilorier's new product enabled him to obtain, combined with great pressures. (Specimens of gases thus liquefied by Faraday shown.) This important work was the subject of a Friday evening lecture given at the Royal Institution early in 1845, a full abstract of which appeared in the *Times* of that date, the Institution itself not having then commenced the publication of its proceedings. In the course of that address Faraday produced a small quantity of ethylene; and he expressed the opinion that if a method could be found of producing liquid nitrous oxide in large quantities, that would be the material which would enable him to liquefy oxygen and the other gases which had hitherto resisted all his efforts. (Experiments showing the comparative boiling points of solid carbonic acid, nitrous oxide, and ethylene at ordinary pressure and under diminished pressure given.) Faraday hoped that the production of solid nitrous oxide would enable him to get temperatures as far below the boiling point of carbonic acid as the temperature of that body was below ordinary temperatures. As a matter of fact, it is impossible to reach such low temperatures by the agency of solid nitrous oxide, and such great depression of temperature was not attained until such time as liquid ethylene became available. The lecturer here showed and described a diagram of the machinery and apparatus now employed at the Royal Institution for the liquefaction and solidification of gases, see Fig. 1. The method of producing liquid ethylene, and of employing it over and over again in the apparatus was described.

The work done in connection with this subject since the time of Faraday, and especially the investigations of Andrews and Van der Waals, had enabled scientists of the present day to calculate exactly the temperature of the boiling point of hydrogen, the gaseous body which has in the liquid state the lowest boiling point of all the elementary substances, and which has up to the present time resisted liquefaction. The temperature of boiling hydrogen would be  $-253^{\circ}\text{C}.$  The lowest point attained by Faraday was  $110^{\circ}\text{C}.$ , and the lowest temperature yet reached was  $-210^{\circ}\text{C}.$



Prof. Dewar then performed the experiment of actually producing liquid oxygen, which was seen to boil quietly when collected in an open vessel at a temperature of  $-180^{\circ}\text{C}$ . The colour was slightly blue, only a few particles of solid matter being visible, which Prof. Dewar explained were traces of solid carbonic acid, the elimination of which had given him considerable trouble. The lecturer further proved by actual experiment on his own hand and on a glass vessel that the liquid oxygen was in the spheroidal condition; and also that alcohol when added to the liquid became instantly solidified. The usual test for oxygen by means of a glowing taper was also made on the vapour given off by the liquid. The form and arrangement of the apparatus employed on the lecture table is shown in Fig. 2.

Prof. Dewar stated that the prosecution of the researches inaugurated by Faraday was enabling scientists to approach nearer and nearer to the zero of absolute temperature; and the speculations of physicists were now directed to the probable characteristics of hydrogen and of matter in general when that condition should be attained. At such a temperature the properties of matter would in all probability be entirely changed; the old Lucretian law would be suspended; molecular motion would probably cease, and what might be called the death of matter would ensue—as in fact the death of chemical affinity and chemical action was known to take place at the low temperatures already attainable. (Experiment proving this by the immersion of phosphorus, sodium, and potassium in liquid oxygen.) On the other hand, it was found that even at such low temperatures oxygen retained its characteristic absorption spectrum.\* Further experiments were given proving the liquefaction of ozone by means of liquid oxygen—a tube of the liquid thus produced showing the characteristic deep blue colour of that substance.

In conclusion, Prof. Dewar said that although great progress had been made since Faraday's time, chemists were still working distinctly on the lines of his early researches; and it seemed to him that no fitter method of celebrating the centenary of Faraday's birth could be chosen than the demonstration of the realisation of some of his own ideas.

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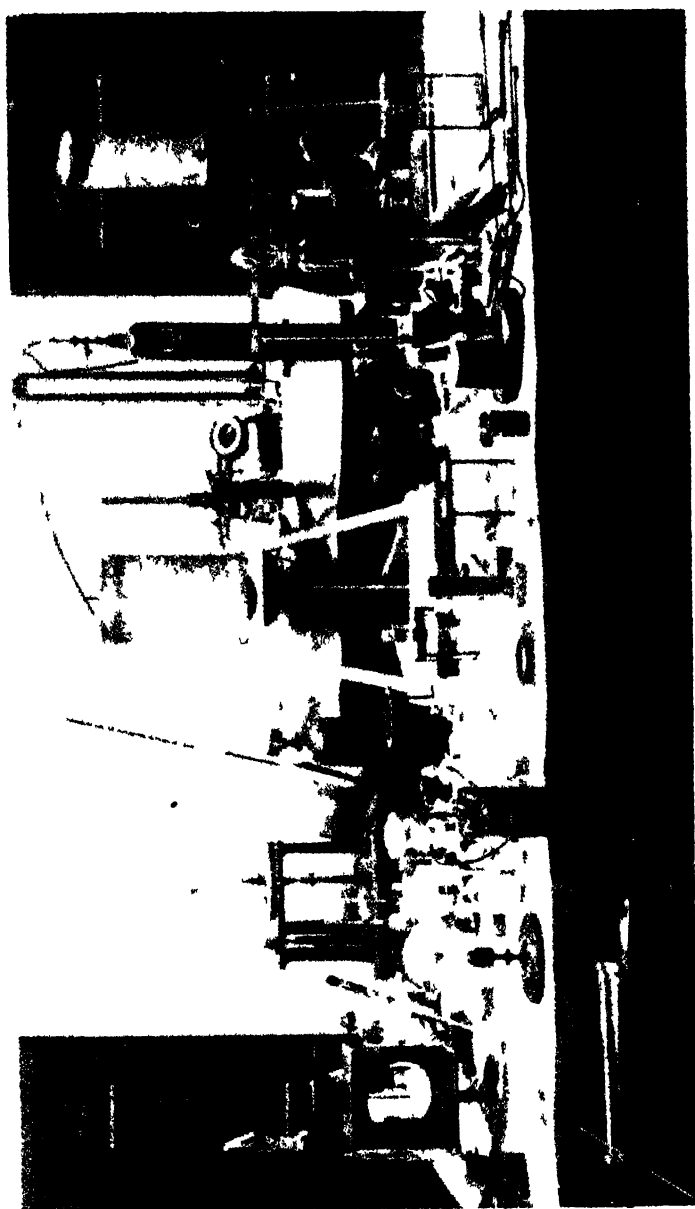
\* The recently discovered magnetic property of the liquid adds a new interest to this substance.

“Royal Institution, 10th December, 1891.

“DEAR SIR WILLIAM THOMSON,—The following observation, which I have just made, may interest the members of the Royal Society, and if you think it of sufficient importance you may announce it at this day's meeting.

“At 3 p.m. this afternoon I placed a quantity of liquid oxygen in the state of rapid ebullition in air (and therefore at a temperature of  $-181^{\circ}\text{C}$ .) between the poles of the historic Faraday magnet, in a cup-shaped piece of rock salt (which I have found is not moistened by liquid oxygen, and therefore keeps it in the spheroidal state), and to my surprise I have witnessed the liquid oxygen, as soon as the magnet was stimulated, *suddenly leap up to the poles and remain there permanently attached until it evaporated.* To see liquid oxygen suddenly attracted by the magnet is a very beautiful confirmation of our knowledge of the properties of gaseous oxygen.—Yours faithfully, JAMES DEWAR.”—[Proc. Royal Society, vol. i. p. 24.]

Fig. 11



On the conclusion of the lecture, a vote of thanks to Prof. Dewar was moved by the LORD CHANCELLOR, who said :—

My Lord Duke, my Lords, Ladies and Gentlemen,—I am very happy indeed to be made the instrument of conveying your thanks for the most interesting lecture we have listened to. I could not help thinking while our lecturer was giving us an account of all these wonderful things, that he was illustrating in his own person something which he had said. He pointed out how the torch of science was passed on from hand to hand, how, for instance, Davy had handed to Faraday some of the sources of those great discoveries which he afterwards disclosed to the world; and I thought that it required some such successor to give adequate expression to the history of Faraday's work. Faraday had many friends; many of us have listened to him in this theatre, as indeed I have had the privilege of doing myself; and I think I may say that no one came within the sphere of his kindly and gentle influence who did not become a hearty and attached friend. But I should think that very few of those friends would be able to give adequate expression to what he had done, the discoveries he had made, and the ever increasing effect which these discoveries had exercised upon the progress of modern science. We have listened to-night to a most able exposition of Faraday's work; and I think that Prof. Dewar has shown that he has in truth succeeded to that work, that he is worthy to receive that torch and carry it on and give a brighter illumination to science than it has ever yet received. I am sure that there is none here who will not heartily join with your Grace in thanking Prof. Dewar for the able, learned and lucid lecture in which he has explained to ignorant people like myself Faraday's wonderful discoveries in science.

Sir LYON PLAYFAIR, in seconding the motion, said: It is indeed a great privilege to all of us to see the great progress which has been made in the discoveries of Faraday during the last fifty years. Those little tubes, containing the original liquefied gases which Faraday liquefied under pressure and low temperatures were very important and were considered at the time very remarkable productions. But you see how the subject has since grown; how carbonic acid, for instance, first liquefied, has since been solidified so that it can be handled like snow; and you have seen the remarkable way in which oxygen has been liquefied on the present occasion. An old Professor of chemistry like myself can appreciate the wonderful manipulative power which Prof. Dewar has displayed this evening. Even in the chemical laboratory, with everything quiet around you, it is difficult to make these experiments successfully, but in a theatre of this kind it is marvellous how everything goes wrong; and if we had not had a manipulator of great accuracy and knowledge, we could not have had the gratification which we have enjoyed this evening. What strikes me as being so excellent in my friend, and much more than friend—for he is the greatest chemist that I ever produced, and I am extremely glad to think that he looks up to his old teacher with

affection—while I look to him with love and honour—what I wanted to say is that I think he has done quite rightly in giving you the scientific side of these wonderful discoveries, and showing you the way in which they are growing and giving us a better knowledge of the condition of matter. When Faraday first made experiments like these, some wisacres said: What is the use of it? Faraday replied; “Will you tell me what is the use of a baby?” But Faraday’s baby has centred around it all the hopes and desires of the parents that produced it, and the State also has shown much interest in its upbringing. The bodies that appear in those tubes have become important factors in the progress and industry of the world. The carbonic acid, which I recollect first seeing as a little globule of acid, is now carried in cylinders filling railway trucks, and is applied to many purposes, some important, others more useful than important. For instance the liquid carbonic acid enables barmaids to get beer up from the cellars below without pumping it; that nitrous oxide which we were so interested in as a condensed gas is now largely used by dentists as a means of extracting our teeth without pain; sulphurous acid will, I am certain, become most important in war, for if you took a brittle shell filled with liquid sulphurous acid and threw it between the decks of a ship it would produce such a stink that everybody would disappear in a moment. The time is coming when other gases will be used in this way. Their importance does not altogether consist in their applications to industry, though they are becoming very important in that way. But their importance is that they are teaching us more of the constitution and properties of matter; it is in that respect that they are becoming so interesting in the eyes of scientific men. I have been extremely interested in watching the production of that liquid oxygen. I looked upon it with great respect, and wondered to see it not covered with a cage as if likely to go off at any moment in a terrific explosion. But it is produced in such a manner that its own cold keeps it down, and so we saw it handled in the most marvellous way as an ordinary liquid. I have the utmost pleasure in seconding the vote of thanks to Prof. DEWAR for the brilliant exposition which he has given us.

The CHAIRMAN then put the motion, and it was carried with acclamation.

Prof. DEWAR in reply said: My Lord Duke, my Lords, Ladies and Gentlemen,—I am exceedingly indebted to you for the very kind way in which you have referred to the labours of the lecturer. I can assure you that it has been a source of great pleasure to me, and that in fact I have had the least part to do. This kind of illustration cannot possibly be given without means of various kinds, and there are several benefactors whom I should like to mention in connection with this lecture. First of all, Dr. Anderson gave the pumps which enabled me to compress and evaporate such volatile bodies; secondly, we require machinery to set those pumps in motion, and somebody to look after it, and that has been supplied by the kindness of Mr. Robert

Wilson, of the well-known firm of Messrs. Crossley, who is always ready and willing to help us; thirdly, as regards the cost of the material used—which has been by no means small—I am indebted to another member and great benefactor of the Institution, namely, Mr. Ludwig Mond, F.R.S. And lastly, but not least, I am indebted to my assistants, Mr. Lennox and Mr. Heath, for the assiduous and self-sacrificing way in which they have laboured in order to make these experiments go successfully. As Sir Lyon Playfair has said, it is comparatively easy to do these things in the quiet of the laboratory, but immensely difficult to get them to go on an occasion like this; and when we consider the long distances over which these highly condensed gases have to be conveyed, and the complex arrangements necessary to avoid all fear of danger, I think you will agree that the benefactors who rendered these arrangements possible are deserving of more credit than the lecturer.

Lord Justice FRY then proposed a vote of thanks to his Grace the Duke of Northumberland for his kindness in presiding over the meeting. In doing so, his Lordship said: While I ask you to tender his Grace your hearty thanks for attending to-night I cannot omit to ask you to thank him also for even greater services. He has presided over this Institution for many years, and has ever shown in its affairs a warm and intelligent interest, and he has been a most liberal benefactor of the Institution. At our ordinary meetings we have no opportunity of expressing our feelings to our benefactors; but on this extraordinary occasion we have that opportunity. I feel that I only express the sentiments of all here when I propose to proffer your warmest thanks to his Grace, not only for presiding this evening, but also for the great debt of gratitude which we owe him for his past services.

Sir RICHARD WEBSTER said: I have the great privilege of being permitted to second the vote of thanks to his Grace. I most heartily endorse all that Lord Justice Fry has said with respect to the eminent services rendered to this Institution by his Grace the Chairman. I also heartily agree with what has been said by previous speakers with respect to the admirable lecture that we have heard to-night, some portion of which will, I hope, remain in my mind and memory, but the immediate effect of which has been to completely paralyse the power of ordinary speech. I feel it a great privilege to have been permitted to take some part in the proceedings, and have the greatest pleasure in seconding the vote proposed by Lord Justice Fry, which I venture to hope may be carried by acclamation.

The vote having been put and carried by acclamation,

The DUKE OF NORTHUMBERLAND said in response: My Lords, Ladies and Gentlemen, I feel somewhat embarrassed on the present occasion, because I had no expectation of, nor did I feel myself entitled to, the vote of thanks you have been so kind as to pass. I should have been wanting in duty if I had not been here to attend the Centenary of the illustrious man whose memory we have met to

celebrato; and I must say I have been amply rewarded by the lectures I have heard, both from Lord Rayleigh and Prof. Dewar. They have almost persuaded me between them that I understand something of this science, which I confess but for them would have seemed impossible. Time is getting on, and I therefore will not detain you longer than to thank you most sincerely, and to ask you to accept this simple expression of my gratitude.

Thursday, February 4, 1892.

SIR FREDERICK BRAMWELL, Bart. D.C.L. F.R.S. Honorary  
Secretary and Vice-President, in the Chair.

NIKOLA TESLA, Esq.

*Alternate Currents of High Potential and High Frequency.*

At the first outset this investigation was taken up with the view of studying the effects of rapidly changing electrostatic and electromagnetic stresses. It was thought, from theoretical considerations, that some useful observations would be made in following up this line of experiment by means of properly constructed apparatus; but the anticipations were by far surpassed, for a number of unexpected phenomena were noted, and some novel facts brought to light, which have opened up a new and promising field of research. Some of the results obtained are of special interest on account of their direct bearing upon the problem of producing an efficient illuminant.

The phenomena which are due to the changing character of the stresses are exalted when the time rate of change is increased, hence the study of these phenomena is much facilitated by the employment of apparatus adapted especially for the purpose of carrying on such investigations. With this object in view, several types of alternators were constructed, capable of giving currents of frequencies from five to ten thousand and even more. Currents of much higher frequencies used in some of these experiments, were obtained by disruptively discharging condensers.

The construction of the alternators offered at first great difficulties. To obtain these frequencies it was necessary to provide several hundred polar projections, which were necessarily small and offered many drawbacks, and this the more as exceedingly high peripheral speeds had to be resorted to. In some of the first machines both armature and field had polar projections. These machines produced a curious noise, especially when the armature was started from the state of rest, the field being charged. The most efficient machine was found to be one with a drum armature, the iron body of which consisted of very thin wire annealed with special care. It was, of course, desirable to avoid the employment of iron in the armature, and several machines of this kind, with moving or stationary conductors, were constructed, but the results obtained were not quite satisfactory, on account of the great mechanical and other difficulties encountered. A few of the machines constructed were described in some periodicals of the past year, notably in the *Electrical Engineer*, New York, March 18, 1891.

The study of the properties of the high frequency currents obtained from these machines is very interesting, as nearly every experiment discloses something new.

Two coils traversed by such a current attract or repel each other with a force which, owing to the imperfection of our sense of touch, seems continuous.

An observation, scarcely foreseen, is that a piece of iron, surrounded by a coil through which the current is passing appears to be continuously magnetised. This apparent continuity might be ascribed to the deficiency of the sense of touch, but there is evidence that in currents of such high frequencies one of the impulses preponderates over the other.

As might be expected, conductors traversed by such currents are rapidly heated, owing to the increase of the resistance, and the heating effects are relatively much greater in the iron.

The hysteresis losses in iron are so great that an iron core, even if finely subdivided, is heated in an incredibly short time. To give an idea, an ordinary iron wire of  $1/16$  inch in diameter inserted within a coil having 250 turns, with a current estimated to be five amperes passing through the coil, becomes within two seconds' time so hot as to scorch wood. Beyond a certain frequency, an iron core, no matter how finely subdivided, exercises a dampening effect, and it was easy to find a point at which the impedance of a coil was not affected by the presence of a core consisting of a bundle of very thin well annealed and varnished iron wires.

Experiments with a telephone, a conductor in a strong magnetic field, or with a condenser or arc, seem to afford certain proof that sounds far above the usually accepted limit of hearing would be perceived if produced with sufficient power.

The arc produced by these currents possesses several interesting features. Usually it emits a note the pitch of which corresponds to twice the frequency of the current, but if the frequency be sufficiently high it becomes noiseless, the limit of audition being determined principally by the linear dimensions of the arc. A curious feature of the arc is its persistency, which is due partly to the inability of the gaseous column to cool and increase considerably in resistance, as in the case with low frequencies, and partly to the tendency of such a high frequency machine to maintain a constant current.

In connection with these machines the condenser affords a particularly interesting study. Striking effects are produced by proper adjustments of capacity and self-induction. It is easy to raise the electro-motive force of the machine to many times the original value by simply adjusting the capacity of a condenser connected in the induced circuit. If the condenser be at some distance from the machine, the difference of potential on the terminals of the latter may be only a small fraction of that on the condenser.

But the most interesting experiences are made when the tension



of the currents from the machine is raised by means of an induction coil. In consequence of the enormous rate of change obtainable in the primary current, much higher potential differences are obtained than with coils operated in the usual ways, and, owing to the high frequency, the secondary discharge possesses many striking peculiarities. Both the electrodes behave generally alike, though it appears from some observations that one current impulse preponderates over the other, as before mentioned.

The physiological effects of the high tension discharge are found to be so small that the shock of the coil can be supported without any inconvenience, except perhaps a small burn produced by the discharge upon approaching the hand to one of the terminals.

The decidedly smaller physiological effects of these currents are thought to be due either to a different distribution through the body or to the tissues acting as condensers. But in the case of an induction coil with a great many turns the harmlessness is principally due to the fact that but little energy is available in the external circuit when the same is closed through the experimenter's body, on account of the great impedance of the coil.

In varying the frequency and strength of the currents through the primary of the coil, the character of the secondary discharge is greatly varied, and no less than five distinct forms are observed:—A weak, sensitive thread discharge, a powerful flaming discharge, and three forms of brush or streaming discharges. Each of these possesses certain noteworthy features, but the most interesting to study are the latter.

Under certain conditions the streams, which are presumably due to the violent agitation of the air molecules, issue freely from all points of the coil, even through a thick insulation. If there is the smallest air-space between the primary and secondary, they will form there and surely injure the coil by slowly warming the insulation. As they form even with ordinary frequencies when the potential is excessive, the air-space must be most carefully avoided.

These high frequency streamers differ in aspect and properties from those produced by a static machine. The wind produced by them is small and should altogether cease if still considerably higher frequencies could be obtained.

A peculiarity is that they issue as freely from surfaces as from points. Owing to this, a metallic vane, mounted in one of the terminals of the coil so as to rotate freely, and having one of its sides covered with insulation, is spun rapidly around. Such a vane would not rotate with a steady potential, but with a high frequency coil it will spin, even if it be entirely covered with insulation, provided the insulation on one side be either thicker or of a higher specific inductive capacity. A Crookes' electric radiometer is also spun around when connected to one of the terminals of the coil but only at very high exhaustion or at ordinary pressures.

There is still another and more striking peculiarity of such a

high frequency streamer, namely, it is hot. The heat is easily perceptible with frequencies of about 10,000, even if the potential is not excessively high. The heating effect is, of course, due to the molecular impacts and collisions. Could the frequency and potential be pushed far enough, then a brush could be produced resembling in every particular a flame and giving light and heat, yet without a chemical process taking place.

The hot brush, when properly produced, resembles a jet of burning gas escaping under great pressure, and it emits an extraordinary strong smell of ozone. The great ozonising action is ascribed to the fact that the agitation of the molecules of the air is more violent in such a brush than in the ordinary streamer of a static machine.

But the most powerful brush discharges were produced by employing currents of much higher frequencies than it was possible to obtain by means of the alternators. These currents were obtained by disruptively discharging a condenser and setting up oscillations. In this manner currents of a frequency of several hundred thousand were obtained.

Currents of this kind produce striking effects. At these frequencies, the impedance of a copper bar is so great that a potential difference of several hundred volts can be maintained between two points of a short and thick bar, and it is possible to keep an ordinary incandescent lamp burning at full candle power by attaching the terminals of the lamp to two points of the bar no more than a few inches apart. When the frequency is extremely high, nodes are found to exist on such a bar, and it is easy to locate them by means of a lamp.

By converting the high tension discharges of a low frequency coil in this manner, it was found practicable to keep a few lamps burning on the ordinary circuit in the laboratory, and by bringing the undulation to a low pitch, it was possible to operate small motors.

This plan likewise allows of converting high tension discharges of one direction in low tension unidirectional currents, by adjusting the circuit so that there are no oscillations. In passing the oscillating discharges through the primary of a specially constructed coil, it is easy to obtain enormous potential differences with only few turns of the secondary.

Great difficulties were at the beginning experienced in producing a successful coil on this plan. It was found necessary to keep all air, or gaseous matter in general, away from the charged surfaces, and oil immersion was resorted to. The wires used were heavily covered with gutta-percha and wound in oil, or the air was pumped out by means of a Sprengel pump.

The general arrangement was the following: → An ordinary induction coil, operated from a low frequency alternator, was used to charge Leyden jars. The jars were made to discharge over a single

or multiple gap through the primary of the second coil. To insure the action of the gap, the arc was blown out by a magnet or air-blast. To adjust the potential in the secondary a small oil condenser was used, or polished brass spheres of different sizes were screwed on the terminals and their distance adjusted.

When the conditions were carefully determined to suit each experiment, magnificent effects were obtained.

Two wires, stretched through the room, each being connected to one of the terminals of the coil, emit streams so powerful that the light from them allows distinguishing the objects in the room; the wires become luminous even if covered with thick and most excellent insulation. When two straight wires, or two concentric circles of wire, are connected to the terminals, and set at the proper distance, a uniform luminous sheet is produced between them. It was possible in this way to cover an area of more than one meter square completely with the streams. By attaching to one terminal a large circle of wire and to the other terminal a small sphere, the streams are focussed upon the sphere, produce a strongly lighted spot upon the same, and present the appearance of a luminous cone. A very thin wire glued upon a plate of hard rubber of great thickness, on the opposite side of which is fastened a tinfoil coating, is rendered intensely luminous when the coating is connected to the other terminal of the coil. Such an experiment can be performed also with low frequency currents, but much less satisfactorily.

When the terminals of such a coil, even of a very small one, are separated by a rubber or glass plate, the discharge spreads over the plate in the form of streams, threads, or brilliant sparks, and affords a magnificent display, which cannot be equalled by the largest coil operated in the usual ways. By a simple adjustment it is possible to produce with the coil a succession of brilliant sparks, exactly like with a Holtz machine.

Under certain conditions, when the frequency of the oscillation is very great, white phantom-like streams are seen to break forth from the terminals of the coil. The chief interesting feature about them is, that they stream freely against the outstretched hand or other conducting object without producing any sensation, and the hand may be approached very near to the terminal without a spark being induced to jump. This is due presumably to the fact that a considerable portion of the energy is carried away or dissipated in the streamers, and the difference of potential between the terminal and the hand is diminished.

It is found in such experiments, that the frequency of the vibration and the quickness of succession of the sparks between the knobs affect to a marked degree the appearance of the streams. When the frequency is very low, the air gives way in more or less the same manner as by a steady difference of potential, and the streams consist of distinct threads, generally mingled with thin sparks, which probably correspond to the successive discharges occurring

between the knobs. But when the frequency is very high, and the arc of the discharge produces a sound which is loud and smooth (which indicates both that oscillation takes place and that the sparks succeed each other with great rapidity), then the luminous streams formed are perfectly uniform. They are generally of a purplish hue, but when the molecular vibration is increased by raising the potential they assume a white colour.

The luminous intensity of the streams increases rapidly when the potential is increased; and with frequencies of only a few hundred thousand, could the coil be made to withstand a sufficiently high potential difference, there is no doubt that the space around a wire could be made to emit a strong light, merely by the agitation of the molecules of the air at ordinary pressure.

Such discharges of very high frequency which render luminous the air at ordinary pressure we have very likely occasion to witness in the Aurora borealis. From many of these experiments it seems reasonable to infer that sudden cosmic disturbances, such as eruptions on the sun, set the electrostatic charge of the earth in an extremely rapid vibration, and produce the glow by the violent agitation of the air in the upper and even in the lower strata. It is thought that if the frequency were low, or even more so if the charge were not at all vibrating, the lower dense strata would break down as in a lightning discharge. Indications of such breaking down have been repeatedly observed, but they can be attributed to the fundamental disturbances, which are few in number, for the superimposed vibration would be so rapid as to not allow a disruptive break.

The study of these discharge phenomena has led to the recognition of some important facts. It was found that gaseous matter must be most carefully excluded from any dielectric which is subjected to great, rapidly-changing electrostatic stresses. Since it is difficult to exclude the gas perfectly when solid insulators are used, it is necessary to resort to liquid dielectrics. When a solid dielectric is used, it matters little how thick and how good it is; if air be present streamers form, which gradually heat the dielectric and impair its insulating power, and the discharge finally breaks through. Under ordinary conditions the best insulators are those which possess the highest specific inductive capacity, but such insulators are not the best to employ when working with these high frequency currents, for in most cases the higher specific inductive capacity is rather a disadvantage. The prime quality of the insulating medium for these currents is continuity. For this reason principally it is necessary to employ liquid insulators, such as oils. If two metal plates, connected to the terminals of the coil, are immersed in oil and set a distance apart, the coil may be kept working for any length of time without a break occurring, or without the oil being warmed, but if air bubbles are introduced, they become luminous; the air molecules, by their impact against the oil, heat it, and after some time cause the insulation to give way. If, instead of the oil, a solid plate of the best

dielectric, even several times thicker than the oil intervening between the metal plates, is inserted between the latter, the air having free access to the charged surfaces, the dielectric invariably is warmed and breaks down.

The employment of the oil is advisable or necessary even with low frequencies, if the potentials are such that streamers form, but only in such cases, as is evident from the theory of the action. If the potentials are so low that streamers do not form, then it is even disadvantageous to employ oil, for it may, principally by confining the heat, be the cause of the breaking down of the insulation.

The exclusion of gaseous matter is not only desirable on account of the safety of the apparatus, but also on account of economy, especially in a condenser, in which considerable waste of power may occur merely owing to the presence of air, if the electric density on the charged surfaces is great.

In the course of these investigations a phenomenon of special scientific interest has been observed. It may be ranked among the brush phenomena, in fact it is a kind of brush which forms at, or near, a single terminal in high vacuum. In a bulb with a conducting electrode, *etc.* if the latter be of aluminium, the brush has only a very short existence, but it can be preserved for a considerable length of time in a bulb devoid of any conducting electrode. To observe the phenomenon it is found best to employ a large spherical bulb having in its centre a small bulb supported on a tube sealed to the neck of the former. The large bulb being exhausted to a high degree, and the inside of the small bulb being connected to one of the terminals of the coil, under certain conditions there appears a misty haze around the small bulb, which, after passing through some stages, assumes the form of a brush, generally at right angles to the tube supporting the small bulb. When the brush assumes this form it may be brought to a state of extreme sensitiveness to electrostatic and magnetic influence. The bulb hanging straight down, and all objects being remote from it, the approach of the observer within a few paces will cause the brush to fly to the opposite side, and if he walks around the bulb it will always keep on the opposite side. It may begin to spin around the terminal long before it reaches that sensitive stage. When it begins to turn around, principally, but also before, it is affected by a magnet, and at a certain stage it is susceptible to magnetic influence to an astonishing degree. A small permanent magnet, with its poles at a distance of no more than two centimetres, will affect it visibly at a distance of two metres, slowing down or accelerating the rotation according to how it is held relatively to the brush.

When the bulb hangs with the globe down, the rotation is always clockwise. In the southern hemisphere it would occur in the opposite direction and on the (magnetic) equator the brush should not turn at all. The rotation may be reversed by a magnet kept at some distance. The brush rotates best, seemingly, when it is at right angles to the lines of force of the earth. It very likely rotates, when at its maximum

speed, in synchronism with the alternations, say 10,000 times a second. The rotation can be slowed down or accelerated by the approach or receding of the observer, or any conducting body, but it cannot be reversed by putting the bulb in any position. Very curious experiments may be performed with the brush when in its most sensitive state. For instance, the brush resting in one position, the experimenter may, by selecting a proper position, approach the hand at a certain considerable distance to the bulb, and he may cause the brush to pass off by merely stiffening the muscles of the arm, the mere change of configuration of the arm and imperceptible displacement being sufficient to disturb the delicate balance. When it begins to rotate slowly, and the hands are held at a proper distance, it is impossible to make even the slightest motion without producing a visible effect upon the brush. A metal plate connected to the other terminal of the coil affects it at a great distance, slowing down the rotation often to one turn a second.

It is hoped that this phenomenon will prove a valuable aid in the investigation of the nature of the forces acting in an electrostatic or magnetic field. If there is any motion which is measurable going on in the space, such a brush would be apt to reveal it. It is, so to speak, a beam of light, frictionless, devoid of inertia.

On account of its marvellous sensitiveness to electrostatic or magnetic disturbances it may be the means of sending signals through submarine cables with any speed, and even of transmitting intelligence at distance without wires.

In operating an induction coil with these rapidly alternating currents, it is astonishing to note, for the first time, the great importance of the relation of capacity, self-induction, and frequency as regards the general result. The combination of these elements produces many curious effects. For instance, two metal plates are connected to the terminals and set at a small distance, so that an arc is formed between them. This arc *prevents* a strong current to flow through the coil. If the arc be interrupted by the interposition of a glass plate, the capacity of the condenser obtained counteracts the self-induction, and a stronger current is made to pass. The effects of capacity are the most striking, for in these experiments, since the self-induction and frequency both are high, the critical capacity is very small, and need be but slightly varied to produce a very considerable change. The experimenter brings his body in contact with the terminals of the secondary of the coil, or attaches to one or both terminals insulated bodies of very small bulk, such as exhausted bulbs, and he produces a considerable rise or fall of potential on the secondary, and greatly affects the flow of the current through the primary coil.

In many of the phenomena observed, the presence of the air, or, generally speaking, of a medium of a gaseous nature (using this term not to imply specific properties, but as contradistinction to homogeneity or perfect continuity) plays an important part, as it allows

energy to be dissipated by molecular impact or bombardment. The action is thus explained :—

When an insulated body connected to a terminal of the coil is suddenly charged to a high potential, it acts inductively upon the surrounding air, or whatever gaseous medium there might be. The molecules or atoms which are near it are, of course, more attracted, and move through a greater distance than the further ones. When the nearest molecules strike the body they are repelled, and collisions occur at all distances within the inductive distance. It is now clear that, if the potential be steady, but little loss of energy can be caused in this way, for the molecules which are nearest to the body having had an additional charge imparted to them by contact, are not attracted until they have parted, if not with all, at least with most of the additional charge, which can be accomplished only after a great many collisions. This is inferred from the fact that with a steady potential there is but little loss in dry air. When the potential, instead of being steady, is alternating, the conditions are entirely different. In this case a rhythmical bombardment occurs, no matter whether the molecules after coming in contact with the body lose the imparted charge or not, and, what is more, if the charge is not lost, the impacts are only the more violent. Still, if the frequency of the impulses be very small, the loss caused by the impacts and collisions would not be serious unless the potential were excessive. But when extremely high frequencies and more or less high potentials are used, the loss may be very great. The total energy lost per unit of time is proportionate to the product of the number of impacts per second, or the frequency and the energy lost in each impact. But the energy of an impact must be proportionate to the square of the electric density of the body, on the assumption that the charge imparted to the molecule is proportionate to that density. It is concluded from this that the total energy lost must be proportionate to the product of the frequency and the square of the electric density; but this law needs experimental confirmation. Assuming the preceding considerations to be true, then, by rapidly alternating the potential of a body immersed in an insulating gaseous medium, any amount of energy may be dissipated into space. Most of that energy, then, is not dissipated in the form of long ether waves, propagated to considerable distance, as is thought most generally, but is consumed in impact and collisional losses—that is, heat vibrations—on the surface and in the vicinity of the body. To reduce the dissipation it is necessary to work with a small electric density—the smaller the higher the frequency.

The behaviour of a gaseous medium to such rapid alternations of potential makes it appear plausible that electrostatic disturbances of the earth, produced by cosmic events, may have great influence upon the meteorological conditions. When such disturbances occur both the frequency of the vibrations of the charge and the potential are in all probability excessive, and the energy converted into heat may be

considerable. Since the density must be unevenly distributed, either in consequence of the irregularity of the earth's surface, or on account of the condition of the atmosphere in various places, the effect produced would accordingly vary from place to place. Considerable variations in the temperature and pressure of the atmosphere may in this manner be caused at any point of the surface of the earth. The variations may be gradual or very sudden, according to the nature of the original disturbance, and may produce rain and storms, or locally modify the weather in any way.

From many experiences gathered in the course of these investigations it appears certain that in lightning discharges the air is an element of importance. For instance, during a storm a stream may form on a nail or pointed projection of a building. If lightning strikes somewhere in the neighbourhood, the harmless static discharge may, in consequence of the oscillations set up, assume the character of a high-frequency streamer, and the nail or projection may be brought to a high temperature by the violent impact of the air molecules. Thus, it is thought, a building may be set on fire without the lightning striking it.

In like manner small metallic objects may be fused and volatilised—as frequently occurs in lightning discharges—merely because they are surrounded by air. Were they immersed in a practically continuous medium, such as oil, they would probably be safe, as the energy would have to spend itself elsewhere.

An instructive experience having a bearing on this subject is the following:—A glass tube of an inch or so in diameter and several inches long is taken, and a platinum wire sealed into it, the wire running through the centre of the tube from end to end. The tube is exhausted to a moderate degree. If a steady current is passed through the wire it is heated uniformly in all parts and the gas in the tube is of no consequence. But if high frequency discharges are directed through the wire, it is heated more on the ends than in the middle portion, and if the frequency, or rate of charge, is high enough, the wire might as well be cut in the middle as not, for most of the heating on the ends is due to the rarefied gas. Here the gas might only act as a conductor of no impedance, diverting the current from the wire as the impedance of the latter is enormously increased, and merely heating the ends of the wire by reason of their resistance to the passage of the discharge. But it is not at all necessary that the gas in the tube should be conducting; it might be at an extremely low pressure, still the ends of the wire would be heated, as, however, is ascertained by experience, only the two ends would in such case not be electrically connected through the gaseous medium. Now what with these frequencies and potentials occurs in an exhausted tube, occurs in the lightning discharge at ordinary pressure.

From the facility with which any amount of energy may be carried off through a gas, it is concluded that the best way to render harm-



loss a lightning discharge is to afford it in some way a passage through a volume of gas.

The recognition of some of the above facts has a bearing upon far-reaching scientific investigations in which extremely high frequencies and potentials are used. In such cases the air is an important factor to be considered. So, for instance, if two wires are attached to the terminals of the coil, and streamers issue from them, there is dissipation of energy in the form of heat and light, and the wires behave like a condenser of larger capacity. If the wires be immersed in oil, the dissipation of energy is prevented, or at least reduced, and the apparent capacity is diminished. The action of the air would seem to make it very difficult to tell, from the measured or computed capacity of a condenser in which the air is acted upon, its actual capacity or vibration period, especially if the condenser is of very small surface and is charged to a very high potential. As many important results are dependent upon the correctness of the estimation of the vibration period, this subject demands the most careful scrutiny of other investigators.

In Leyden jars the loss due to the presence of air is comparatively small, principally on account of the great surface of the coatings and the small external action, but if there are streamers on the top, the loss may be considerable, and the period of vibration is affected. In a resonator, the density is small, but the frequency is extreme, and may introduce a considerable error. It appears certain, at any rate, that the periods of vibration of a charged body in a gaseous and in a continuous medium, such as oil, are different, on account of the action of the former, as explained.

Another fact recognised, which is of some consequence, is, that in similar investigations the general considerations of static screening are not applicable when a gaseous medium is present. This is evident from the following experiment.

A short and wide glass tube is taken and covered with a substantial coating of bronze, barely allowing the light to shine a little through. The tube is highly exhausted and suspended on a metallic clasp from the end of a wire. When the wire is connected with one of the terminals of the coil, the gas inside of the tube is lighted in spite of the metal coating. Here the metal evidently does not screen the gas inside as it ought to, even if it be very thin and poorly conducting. Yet, in a condition of rest the metal coating, however thin, screens the inside perfectly.

One of the most interesting results arrived at in pursuing these experiments, is the demonstration of the fact that a gaseous medium, upon which vibration is impressed by rapid changes of electrostatic potential, is rigid. In illustration of this result an experiment may be cited.

A glass tube about 1 inch in diameter and 3 feet long, with outside condenser coatings on the ends, was exhausted to a certain point, when, the tube being suspended freely from a wire connecting the

upper coating to one of the terminals of the coil, the discharge appeared in the form of a luminous thread, passing through the axis of the tube. Usually the thread was sharply defined in the upper part of the tube and lost itself in the lower part. When a magnet or the finger was quickly passed near the upper part of the luminous thread, it was brought out of position by magnetic or electrostatic influence, and a transversal vibration like that of a suspended cord, with one or more distinct nodes, was set up, which lasted for a few minutes and died gradually out. By suspending to the lower condenser coating metal plates of different sizes, the speed of the vibration was varied. This vibration would seem to show beyond doubt that the thread possessed rigidity, at least to transversal displacements.

Many experiments were tried to demonstrate this property in air at ordinary pressure. Though no positive evidence has been obtained, it is thought nevertheless, that a high frequency brush or streamer, if the frequency could be pushed far enough, would be decidedly rigid. A small sphere might then be moved within it quite freely, but if thrown against it the sphere would rebound. An ordinary flame cannot possess rigidity to a marked degree because the vibration is directionless; but an electric arc, it is believed, must possess that property more or less. A luminous band excited in a bulb by repeated discharges of a Leyden jar must also possess rigidity, and if deformed and suddenly released should vibrate.

From like considerations other conclusions of interest may be made. The most probable medium filling the space is one consisting of independent carriers immersed in an insulating fluid. If through this medium enormous electrostatic stresses are assumed to act, which vary rapidly in intensity, it would allow the motion of a body through it, yet it would be rigid and elastic, although the fluid itself might be devoid of these properties. Furthermore, on the assumption that the independent carriers are of any configuration such that the fluid resistance to motion in one direction is greater than in another, a stress of that nature would cause the carriers to arrange themselves in groups, since they would turn to each other their sides of the greatest electric density, in which position the fluid resistance to approach would be smaller than to receding. If in a medium of the above characteristics a brush would be formed by a steady potential, an exchange of the carriers would go on continually, and there would be less carriers per unit of volume in the brush than in the space at some distance from the electrode, this corresponding to rarefaction. If the potential were rapidly changing, the result would be very different: the higher the frequency of the pulses, the slower would be the exchange of the carriers; finally, the motion of translation through measurable space would cease, and, with a sufficiently high frequency and intensity of the stress, the carriers would be drawn towards the electrode, and compression would result.

An interesting feature of these high frequency currents is that

they allow to operate all kinds of devices by connecting the device with only one leading wire to the source. In fact, under certain conditions it may be more economical to supply the electrical energy with one lead than with two.

An experiment of special interest is the running, by the use of only one insulated line, of a motor operating on the principle of the rotating magnetic field enunciated by the author a few years ago. A simple form of such a motor is obtained by winding upon a laminated iron core a primary and close to it a secondary coil, closing the ends of the latter and placing a freely movable metal disk within the influence of the moving field. The secondary coil may, however, be omitted. When one of the ends of the primary coil of the motor is connected to one of the terminals of the high-frequency coil and the other end to an insulated metal plate, which, it should be stated, is not absolutely necessary for the success of the experiment, the disk is set in rotation.

Experiments of this kind seem to bring it within the reach of possibility to operate a motor at any point of the earth's surface from a central source, without any connection to the same except through the earth. If, by means of powerful machinery, rapid variations of the earth's potential were produced, a grounded wire reaching up to some height would be traversed by a current which could be increased by connecting the free end of the wire to a body of some size. The current might be converted to low tension and used to operate a motor or other device. The experiment, which would be one of great scientific interest, would probably best succeed on a ship at sea. In this manner, even if it were not possible to operate machinery, intelligence might be transmitted quite certainly.

In the course of this experimental study special attention was devoted to the heating effects produced by these currents, which are not only striking, but open up the possibility of producing a more efficient illuminant. It is sufficient to attach to the coil terminal a thin wire or filament, to have the temperature of the latter perceptibly raised. If the wire or filament be inclosed in a bulb, the heating effect is increased by preventing the circulation of the air. If the air in the bulb be strongly compressed, the displacements are smaller, the impacts less violent, and the heating effect is diminished. On the contrary, if the air in the bulb be exhausted, an inclosed lamp filament is brought to incandescence, and any amount of light may thus be produced.

The heating of the inclosed lamp filament depends on so many things of a different nature, that it is difficult to give a generally applicable rule under which the maximum heating occurs. As regards the size of the bulb, it is ascertained that at ordinary or only slightly differing atmospheric pressures, when air is a good insulator, the filament is heated more in a small bulb, because of the better confinement of heat in this case. At lower pressures, when air becomes conducting, the heating effect is greater in a large bulb,

but at excessively high degrees of exhaustion there seems to be, beyond a certain and rather small size of the vessel, no perceptible difference in the heating.

The shape of the vessel is also of some importance, and it has been found of advantage for reasons of economy to employ a spherical bulb with the electrode mounted in its centre, where the rebounding molecules collide.

It is desirable on account of economy that all the energy supplied to the bulb from the source should reach without loss the body to be heated. The loss in conveying the energy from the source to the body may be reduced by employing thin wires heavily coated with insulation, and by the use of electrostatic screens. It is to be remarked, that the screen cannot be connected to the ground as under ordinary conditions.

In the bulb itself a large portion of the energy supplied may be lost by molecular bombardment against the wire connecting the body to be heated with the source. Considerable improvement was effected by covering the glass stem containing the wire with a closely fitting conducting tube. This tube is made to project a little above the glass, and prevents the cracking of the latter near the heated body. The effectiveness of the conducting tube is limited to very high degrees of exhaustion. It diminishes the energy lost in bombardment for two reasons: firstly, the charge given up by the atoms spreads over a greater area, and hence the electric density at any point is small, and the atoms are repelled with less energy than if they would strike against a good insulator; secondly, as the tube is electrified by the atoms which first come in contact with it, the progress of the following atoms against the tube is more or less checked by the repulsion which the electrified tube must exert upon the similarly electrified atoms. This, it is thought, explains why the discharge through a bulb is established with much greater facility when an insulator than when a conductor is present.

During the investigations a great many bulbs of different construction, with electrodes of different material, were experimented upon, and a number of observations of interest were made.

It was found that the deterioration of the electrode is the less the higher the frequency. This was to be expected, as then the heating is effected by many small impacts, instead of by fewer and more violent ones, which shatter quickly the structure. The deterioration is also smaller when the vibration is harmonic. Thus an electrode, maintained at a certain degree of heat, lasts much longer with currents obtained from an alternator, than with those obtained by means of a disruptive discharge. One of the most durable electrodes was obtained from strongly compressed carborundum, which is a kind of carbon recently produced by Mr. E. G. Acheson. From experience, it is inferred, that to be most durable, the electrode should be in the form of a sphere with a highly polished surface.

In some bulbs refractory bodies were mounted in a carbon cup

and pushed under the molecular impact. It was observed in such experiments that the carbon cup was heated at first, until a higher temperature was reached; then most of the bombardment was directed against the refractory body, and the carbon was relieved. In general, when different bodies were mounted in the bulb, the hardest fusible would be relieved, and would remain at a considerably lower temperature. This was necessitated by the fact that most of the energy supplied would find its way through the body which was easier fused or "evaporated."

Curiously enough it appeared in some of the experiments made, that a body was fused in a bulb under the molecular impact by evolution of less light than when fused by the application of heat in ordinary ways. This may be ascribed to a loosening of the structure of the body under the violent impacts and changing stresses.

Some experiences seem to indicate that under certain conditions a body, conducting or nonconducting, may, when bombarded, emit light, which to all appearance is due to phosphorescence, but may in reality be caused by the incandescence of an infinitesimal layer, the mean temperature of the body being comparatively small. Such might be the case if each single rhythmical impact were capable of instantaneously exciting the retina, and the rhythm just high enough to cause a continuous impression in the eye. According to this view, a coil operated by disruptive discharge would be eminently adapted to produce such a result, and it is found in experience that its power of exciting phosphorescence is extraordinarily great. It is capable of exciting phosphorescence at comparatively low degrees of exhaustion, and also projects shadows at pressures far greater than those at which the mean free path is comparable to the dimensions of the vessel. The latter observation is of some importance, inasmuch as it may modify the generally accepted views in regard to the "radiant state" phenomena.

A thought, which early and naturally suggested itself, was to utilise the great inductive effects of high frequency currents to produce light in a sealed glass vessel without the use of leading-in wires. Accordingly, many bulbs were constructed in which the energy necessary to maintain a button or filament at high incandescence, was supplied through the glass either by electrostatic or electrodynamic induction. It was likewise easy to regulate the intensity of the light emitted by means of an externally applied condenser coating connected to an insulated plate, or simply by means of a plate attached to the bulb which at the same time performed the function of a shade.

A subject of experiment, which has been exhaustively treated by Prof. J. J. Thomson, has been followed up independently by the author from the beginning of this study, namely, to excite by electrodynamic induction a luminous band in a closed tube or bulb. In observing the behaviour of gases, and the luminous phenomena obtained, the importance of the electrostatic effects was noted and it

appeared desirable to produce enormous potential differences, alternating with extreme rapidity. Experiments in this direction led to some of the most interesting results arrived at in the course of these investigations. It was found that by rapid alternations of a high electrostatic potential, exhausted tubes could be lighted at considerable distance from a conductor connected to a properly constructed coil, and that it was practicable to establish with the coil an alternating electrostatic field, acting through the whole extent of a room and lighting a tube, wherever it was placed in the same. Phosphorescent bulbs may be excited in such a field, and it is easy to regulate the effect by connecting to the bulb a small insulated metal plate. It was likewise possible to maintain a filament or button mounted in a tube at bright incandescence, and in one experiment, a mica vane was spun by the incandescence of a platinum wire.

It is hoped that the study of these phenomena, and the perfection of the means for obtaining rapidly alternating high potentials, will lead to the production of an efficient illuminant.

[NIKOLA TESLA.]

Friday, February 26, 1892.

SIR FREDERICK BRAMWELL, Bart. D.C.L. F.R.S. Honorary Secretary  
and Vice-President, in the Chair.

SIR DAVID SALOMONS, Bart. M.A. M.R.I.

*Optical Projection.*

THE intention of this lecture is to give a general survey of the subject of Optical Projection, which now takes its position in science, and to present examples of what may be done by this method. It would be difficult to determine which subject claims a first place. Some scientists say the microscope should have the preference, while others take a different view. For my own part, I think the microscope and polariscope stand foremost, on account of the facility with which these branches of science may be pursued for the benefit of a large number, without multiplying expensive apparatus; also because of the convenience in saving the eyes from undue strain. Indeed, to many persons, looking at objects in the table microscope is little short of a painful operation, and consequently the study of small objects becomes to them impossible. The projection method immediately brings the required relief.

For general instruction, projection methods are invaluable, such as for instance, showing diagrams, photographs, and other slides, upon the screen; as well as for spectrum analysis. In fact, the subjects which can be illustrated by means of optical projection are innumerable; but time will allow me to present only a few examples, and I trust that, when I approach the end of my lecture, my view of the importance of this subject will be held in equal estimation by you.

Probably the only people in the world that benefit by the experience of their predecessors are those who pursue the study of science. They are free from the accusation of robbing the brains of other men, when they take up methods or apparatus already known and improve upon them or employ them for their own work. In such cases, however, it is always understood that honour should be given where honour is due, and accordingly I have no wish to represent to you any piece of apparatus as of my own devising, when in reality it belongs to another.

Few men have had a larger experience, and attained greater success in optical projection, than has Mr. Lewis Wright, who has embodied in his most recent forms of apparatus all that was good in designs existing until his time. I have, therefore, started from his models, making such modifications as I thought to be desirable.

Mr. Wright does not appear—if I may say so—to have had much experience with the electric arc light as a radiant, and I found, at a very early stage, that great difficulties had to be encountered when this light was used, chiefly because the radiant approaches more nearly to what theory requires. That which was easy with the lime light became almost impossible with the arc lamp, and these difficulties had to be conquered.

Many scientific men are dissatisfied with the projection microscope, on the ground that very high magnification does not give that resolution and that sharpness which is found in the usual methods of observation. This want I fully admit. At the same time it is scarcely right to condemn a particular method, because you try to apply it to an unsuitable purpose. Hundreds of thousands of subjects may be shown with the projection microscope with far greater profit to the student than was possible in the old way. The very fact that the professor can place his pointer upon any part of the picture on the screen is invaluable to the students. I shall, therefore, attempt to show you only a series of microscopical subjects suitable for projection, and shall not employ very high magnification.

In regard to some substances very high powers may be used with advantage, but much time would be lost in getting them into the field and focussing them upon the screen. These, consequently, I omit, so that a larger number of subjects may be illustrated.

It is fair to state that most of the apparatus used to-night has been constructed by Messrs. Newton, of Fleet Street, and the luminous pointer by Messrs. Steward, of the Strand. The arc lamp is a Brockie's projector. Messrs. Baker, Watson, and others, have also come to my assistance.

I will first show, on the screen, a picture of the lantern carrying its various apparatus; and then a few systems of lenses, which may be employed for the projection microscope, as well as a diagram of the microscope itself.

Sub-stage condensers and objectives are, as a rule, made to suit the table microscope. When projecting, by means of an objective alone, in consequence of the screen distance being very great—or, in other words, the microscope tube being exceedingly long as compared with the table instrument—the objective has to be approached very close to the slide; in fact, with the higher powers, closer than the cover-glass will allow. This close working distance renders necessary special sub-stage condensers, and in many cases a special one is required for every screen distance with each objective. This requisite would seem to be a complete stumbling-block to microscope projection work. With the limelight the difficulty does not enter in the same degree as with the arc light, and as we are now dealing with the latter, further reference need not be made to the oxy-hydrogen light. There are two ways of surmounting the difficulty; one by the use of plano-concave lenses, introduced in such a way as to be equivalent to greatly lengthening the focus of the objective on the screen side,



while it enables, as a consequence, the objective to be slightly further removed from the slide; i.e. giving what is termed a greater working distance. The objection to this method is that, even when these plano-concave lenses are corrected, the result, though greatly improved, is not perfect. The second way, which is a perfect one, is that of introducing an eye-piece. In both these methods, that the best results may be obtained, the objective is made to occupy a position not very different from that which it would do if employed on the table microscope.

In the eye-piece method almost the exact conditions can be complied with for which the objective was made. I propose, therefore, to show the subjects by the eye-piece method. The only objectives which will be used are: (1) Zeiss's 35 millimetre projection objective, with a sub-stage condenser, 4 inches focal length, placed a considerable distance from the slide; (2) Newton's 1-inch projection objective, the sub-stage condenser as in the first case; and (3) Zeiss's  $\frac{1}{4}$ -inch achromatic objective, the sub-stage condenser being Professor Abbe's three-lens condenser with the front lens removed. In all three cases the eye-pieces used are Zeiss Huyghens No. 2 and No. 3.

In each instance I will mention the magnification in diameters, as well as the number of times when reckoned by area, for the appreciation of those who estimate by area; and I will also give the size to which a penny postage-stamp would be increased, supposing it to be made of indiarubber, and stretchable to any extent in all directions. In presenting these figures I do not pretend that they are absolutely correct, but as they have been ascertained under conditions similar to those now existing the errors will not be very great.

In consequence of the field not being quite flat, and the sections having a certain thickness, although extremely thin in most cases, the whole of the object cannot be in focus upon the screen at the same time. By shifting the focussing screw slightly all parts may be brought into focus successively. So-called greater depth of focus is obtained by using an increased working distance; and for projection work over-correction for flatness can alone give a sharp picture all over with very considerable depth of focus; the difficulty of over-correction being that, unless extreme care is taken, certain forms of distortion may be introduced. By stopping down the objective greater flatness of field may be secured, but at the expense of light. There is thus a choice of difficulties, and the least one should be taken.

Turning now to the polariscope. Polarized light teaches us a great deal concerning the structure of matter; it is also a means of confirming the undulatory theory of light. This subject is so large that no attempt can be made to give even a general idea of the field it covers, and the experiments, which will be shown in the polariscope, may be taken simply as a few illustrations of the subject and nothing more; but they will, at any rate, be suggestive of the large field to which this method of analysis can be applied. A vast amount of mathematical proof can be illustrated graphically by various experi-

ments with polarized light. I will show on the screen a diagram of the polariscope. (Shown.)

With reference to showing the spectrum. The method of projecting a spectrum, I think, is new, as I have not seen it described anywhere. It gives practically a direct spectrum with an ordinary prism, without turning the lantern round to an angle with the screen; and here is a diagram of the method.

The details of the apparatus, as well as those of the methods of working, I have modified in almost every instance, for five reasons:— (1) That more certain results may be ensured; (2) that rapidity may be obtained; (3) that only one operator may be needed; (4) that, as far as possible, all parts of the apparatus may be interchangeable and (5) that loose screws and pieces may be dispensed with.

There were then shown by projection a number of slides illustrating various microscopic optical systems, and a number of microscopic slides, followed by a series of general polariscopic projections, some of them to illustrate the strains existing in many forms of matter; also a spectrum by a carbon disulphide prism, in conjunction with a reflecting prism and with a mirror, which, apart from any other result, demonstrates that the loss of light with a reflecting prism is less than with an ordinary glass mirror. Slides and other projections were also thrown upon the screen.

The details are as follows:—

*The Microscope.*—Screen distance, 21 feet. First, 35 millimetres Zeiss projection objective, 4-inch sub-stage condenser, Zeiss Huyghens eye-piece 2; 500 diameters = 250,000 times = penny stamp stretched to cover about 117 square yards. Subjects shown. proboscis of blow-fly; permanent molar displacing milk-tooth (kitten); human scalp, vertical; human scalp, surface; fossil ammonites and belemnite. Second, 1 inch Newton's projection objective, 4-inch sub-stage condenser, Zeiss Huyghens eye-piece 2; 1000 diameters = 1,000,000 times = stamp stretched to about 588 square yards. Objects shown: proboscis of blow-fly; foot of a caterpillar; section of human skin, showing the sweat ducts; phylloxera vastatrix of the vine. Third, 1 inch Newton's projection objective, 4-inch sub-stage condenser, Zeiss Huyghens eye-piece 3; 1300 diameters = 1,690,000 times = stamp stretched to about one-fifth of an acre. Slides shown: proboscis of blow-fly; wings of bee (showing hooklets and ridge); sting of bee (showing the two stings, sheath, and poison-sack); sting of wasp (showing same as last slide); eye of beetle (showing the facets). Fourth,  $\frac{1}{4}$ -inch Zeiss's achromatic objective; Abbé's 3-lens sub-stage condenser, with top lens removed; Zeiss Huyghens eye-piece 3; 4500 diameters = 20,250,000 times = stamp extended to nearly  $2\frac{1}{2}$  acres. Slides shown, proboscis of blow-fly; hair of reindeer (showing cell structure); hair of Indian bat (showing the peculiar funnel-like structure); sting of bee (showing the barbs); foot of spider; stage of the micrometer (the closest lines ruled to thousandths of an inch, which

measure  $4\frac{1}{2}$  inches apart under this magnification); a wave length  $\frac{1}{40000}$  inch, therefore, on screen measures about  $\frac{1}{3}$  inch.

*The Polariscopes.*—Shown with parallel light; plain glass; glass under pressure; chilled glass (round, oval, and waved peripheries); Prince Rupert's drop (broken in the field); horn; selenites (overlapped); butterfly (selenite); bunch of grapes (selenite); bi-quartz, with  $\frac{1}{4}$ -wave plate (the  $\frac{1}{4}$ -wave plate in this experiment produces the same effect upon the bi-quartz as if a column, 20 centimetres long, of a  $7\frac{1}{2}$  per cent. solution of cane sugar were placed between the polarising nicol and the bi-quartz. The analyser has to be rotated about  $10^\circ$ ); a piece of sapphire to show asterism. Shown with convergent light; hemitrope (cut in a plane, not at right angles to the axis); ruby; topaz; grape sugar (diabetic); cane sugar; quartz; superposed right and left-handed quartz (spirals); calcite and phenakite superposed (showing transition from negative to positive crystal, passing through the apopholite stage).

*The Solidiscope.*—New form of apparatus for showing solids, and consisting of two reflecting prisms and suitable projecting lenses. With this instrument were shown:—Barton's button, the works of a watch, a coin.

*Spectrum Analysis.*—Spectrum thrown by means of a disulphide prism, combined with a reflecting prism; the result being that a good spectrum is thrown upon the screen direct without turning the lantern. There were shown:—The spectrum; absorption bands of chlorophyll, &c.; effects produced by passing the light through coloured gelatine films.

*Projection of Slides.*—Decomposition of water; expansion of a wire by means of heat; combination of colours to form white light; various diagrams, coloured photographs of a workshop, &c. As an extra experiment there was shown, in the polariscope, with a convergent light, Mitscherlich's experiment (illustrating the changes which take place in a selenite under the influence of heat).

There are but few who would disagree with me in the opinion that the microscopic world, as regards its design and its molecular structure, is quite as wonderful as the great works around us seen with the unaided eye. A magnifying glass of low power opens up a world far larger than that which we are accustomed to see. At the present time, even with the most perfect apparatus that exist, only a small portion of the universe is known to us.

Scientific study should be pursued by all in a greater or less degree. It teaches more important lessons than the most impressive discourse ever preached. During the investigation of what is generally termed the invisible world, men should at times pause to reflect, and ask themselves such questions as these: What is the meaning of, and to what end is, creation? Is it all mere chance? Were such wonderful designs and properties created at the beginning? Was there in matter at the beginning an inherent, or implanted,

power of development? Simple as these questions may seem, man in the flesh will never be able to find the true answers. The extraordinary design and structure which have existed in the unseen world for millions of years, or possibly in all past time, and even at the present day known to so few, demonstrate at least that the Great Power has bestowed the same care upon what appear to us the most insignificant portions of creation, as upon what we think are the greatest works in the universe. These silent sermons must surely influence the mind, and set it thinking of the supernatural and of our duties during life.

It may now with truth be said that science gives us means, such as never before existed, of appreciating the greatness of the Supreme Spirit, by enabling us to read fresh chapters in the book of nature.

[D. S.]

Friday, April 1, 1892.

WILLIAM HUGGINS, Esq. D.C.L. LL.D. Ph.D. F.R.S. Vice-President,  
in the Chair.

PROFESSOR OLIVER LODGE, D.Sc. LL.D. F.R.S. F.R.A.S.

*The Motion of the Ether near the Earth.*

EVERYBODY knows that to shoot a bird on the wing you must aim in front of it. Every one will readily admit that to hit a squatting rabbit from a moving train you must aim behind it.

These are examples of what may be called "aberration" from the sender's point of view, from the point of view of the source. And the aberration, or needful divergence between the point aimed at and the thing hit has opposite sign in the two cases—the case when receiver is moving, and the case when source is moving. Hence, if both be moving, it is possible for the two aberrations to neutralise each other. So to hit a rabbit running alongside the train you must aim straight at it.

If there were no air that is all simple enough. But every rifleman knows to his cost that though he fixes both himself and his target tightly to the ground, so as to destroy all aberration proper, yet a current of air is very competent to introduce a kind of spurious aberration of its own, which may be called windage; and that he must not aim at the target if he wants to hit it, but must aim a little in the eye of the wind.

So much from the shooter's point of view. Now attend to the point of view of the target.

Consider it made of soft enough material to be completely penetrated by the bullet, leaving a longish hole wherever struck. A person behind the target, whom we may call a marker, by applying his eye to the hole immediately after the hit, may be able to look through it at the shooter, and thereby to spot the successful man. I know that this is not precisely the function of an ordinary marker, but it is more complete than his ordinary function. All he does usually is to signal an impersonal hit; some one else has to record the identity of the shooter. I am rather assuming a volley of shots, and that the marker has to allocate the hits to their respective sources by means of the holes made in the target.

Well, will he do it correctly? assuming, of course, that he can do so if everything is stationary, and ignoring all curvature of path,

whether vertical or horizontal curvature. If you think it over you will perceive that a wind will not prevent his doing it correctly ; the line of hole will point to the shooter along the path of his bullet, though it will not point along his line of aim. Also, if the shots are fired from a moving ship, the line of hole in a stationary target will point to the position the gun occupied at the instant the shot was fired, though it may have moved since then. In neither of these cases (moving medium and moving source) will there be any aberration error.

But if the *target* is in motion, on an armoured train for instance, then the marker will be at fault. The hole will not point to the man who fired the shot, but to an individual ahead of him. The source will appear to be displaced in the direction of the observer's motion. This is common aberration. It is the simplest thing in the world. The easiest illustration of it is that when you run through a vertical shower, you tilt your umbrella forward ; or, if you have not got one, the drops hit you in the face ; more accurately, your face as you run forward hits the drops. So the shower appears to come from a cloud ahead of you, instead of from one overhead.

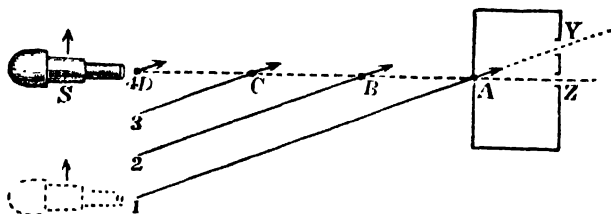
We have thus three motions to consider, that of the source, of the receiver, and of the medium ; and of these only motion of receiver is able to cause an aberrational error in fixing the position of the source.

So far we have attended to the case of projectiles, with the object of leading up to light. But light does not consist of projectiles, it consists of waves ; and with waves matters are a little different. Waves crawl through a medium at their own definite pace ; they cannot be *flung* forwards or sideways by a moving source ; they do not move by reason of an initial momentum which they are gradually expending, as shots do ; their motion is more analogous to that of a bird or other self-propelling animal than it is to that of a shot. The motion of a wave in a moving medium may be likened to that of a rowing boat on a river. It crawls forward with the water, and it drifts with the water ; its resultant motion is compounded of the two, but it has nothing to do with the motion of its source. A shot from a passing steamer retains the motion of the steamer as well as that given it by the powder. It is projected therefore in a slant direction. A boat lowered from the side of a passing steamer, and rowing off, retains none of the motion of its source ; it is not projected, it is self-propelled. That is like the case of a wave.

The diagram illustrates the difference. Fig. 1 shows a moving cannon or machine-gun, moving with the arrow, and firing a succession of shots which share the motion of the cannon as well as their own, and so travel slant. The shot fired from position 1 has reached A, that fired from the position 2 has reached B, and that fired from position 3 has reached C by the time the fourth shot is fired at D. The line A B C D is a prolongation of the axis of the gun ; it is the line of aim, but it is not the line of fire ; all the shots are travelling aslant

this line, as shown by the arrows. There are thus two directions to be distinguished. There is the row of successive shots, and there is the path of any one shot. These two directions enclose an angle. It may be called an aberration angle, because it is due to the motion of the source, but it need not give rise to any aberration. True direction may still be perceived from the point of view of the receiver. Attend

FIG. 1.

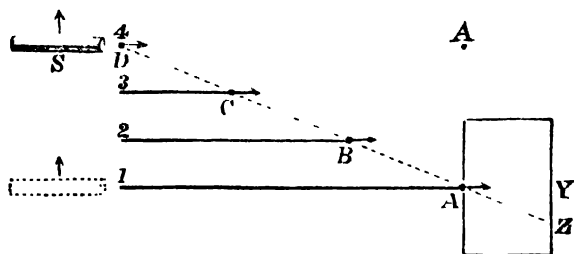


Disturbances with Momentum.

to the target. The first shot is supposed to be entering at A, and if the target is stationary will leave it at Y. A marker looking along YA will see the position whence the shot was fired. This may be likened to a stationary observer looking at a moving star. He sees it where and as it was when the light started on its long journey. He does not see its present position, but there is no reason why he should. He does not see its physical state or anything as it is now. There is no aberration caused by motion of source.

But now let the receiver be moving at same pace as the gun, as when two grappled ships are firing into each other. The motion of the target carries the point Y forward, and the shot A leaves it at Z, because Z is carried to where Y was. So in that case the marker looking along ZA will see the gun, not as it was when firing, but as it is at the present moment; and he will see likewise the row of shots

FIG. 2.



Disturbances without Momentum.

making straight for him. This is like an observer looking at a terrestrial object. Motion of the earth does not disturb ordinary vision.

Fig. 2 shows as nearly the same sort of thing as possible for the

case of emitted waves. The tube is a source emitting a succession of disturbances without momentum.  $ABCD$  may be thought of as horizontally flying birds, or as crests of waves; or they may even be thought of as bullets, if the gun stands still every time it fires, and only moves between whiles.

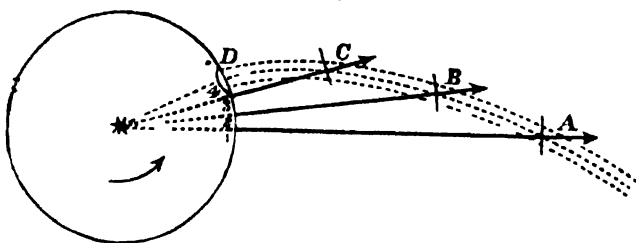
The line  $ABCD$  is now neither the line of fire nor the line of aim: it is simply the locus of disturbances emitted from the successive positions 1 2 3 4.

A stationary target will be penetrated in the direction  $AY$ , and this line will point out the correct position of the source when the received disturbance started. If the target moves, a disturbance entering at  $A$  may leave it at  $Z$ , or at any other point according to its rate of motion; the line  $ZA$  does not point to the source, and so there will be aberration when the target moves. Otherwise there would be none.

Now Fig. 2 also represents a parallel beam of light travelling from a moving source, and entering a telescope or the eye of an observer. The beam lies along  $ABCD$ , but this is not the direction of vision. The direction of vision to a stationary observer is determined not by the locus of successive waves, but by the path of each wave. A ray may be defined as the path of a labelled disturbance. The line of vision is  $YA$ , and coincides with the line of aim; which in the projectile case (Fig. 1) it did not.

The case of a revolving lighthouse, emitting long parallel beams of light and brandishing them rapidly round, is rather interesting. Fig. 3 may assist the thinking out of this case. Successive dis-

FIG. 3.



Beam from a Revolving Lighthouse

turbances  $A, B, C, D$ , lie along a spiral curve, the spiral of Archimedes; and this is the shape of the beams as seen illuminating the dust particles, though the pitch of the spiral is too gigantic to be distinguished from a straight line. At first sight it might seem as if an eye looking along those curved beams would see the lighthouse slightly out of its true position; but it is not so. The true rays or actual paths of each disturbance are truly radial; they do not coincide with the apparent beam. An eye looking at the source will not look tangentially along the beam, but will look along  $AS$ , and will see the



source in its true position. It would be otherwise for the case of projectiles from a revolving turret.

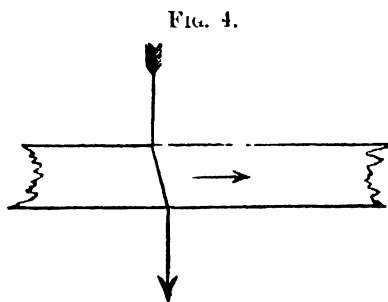
Thus, neither translation of star nor rotation of sun can affect direction. There is no aberration so long as the receiver is stationary.

But what about a wind, or streaming of the medium past source and receiver, both stationary? Look at Fig. 1 again. Suppose a row of stationary cannon firing shots, which get blown by a cross wind along the slant  $1 A Y$  (neglecting the curvature of path which would really exist): still the hole in the target fixes the gun's true position, the marker looking along  $Y A$  sees the gun which fired the shot. There is no true deviation from the point of view of the receiver, although the shots are blown aside and the target is not hit by the particular gun aimed at it.

With a moving cannon, combined with an opposing wind, Fig. 1 would become very like Fig. 2.

(N.B.—The actual case, even without complication of spinning, &c., but merely with the curved path caused by steady wind-pressure, is not so simple, and there would really be an aberration or apparent displacement of the source towards the wind's eye: an apparent exaggeration of the effect of wind as shown in the diagram.)

In Fig. 2 the result of a wind is much the same, though the details are rather different. The medium is supposed to be drifting down across the field opposite to the arrows. The source is stationary at  $S$ . The arrows show the direction of waves *in the medium*; the dotted slant line shows their resultant direction. A wave centre drifts from  $D$  to  $1$  in the same time as the disturbance reaches  $A$ , travelling down the slant line  $D A$ . The angle between dotted and full lines is the angle between ray and wave movement. Now, *if the motion of the medium inside the receiver is the same as it is outside*, the wave will pass straight on along the slant to  $Z$ , and the true direction of the source is fixed. But if the medium inside the target or telescope is stationary, the wave will cease to drift as soon as it gets inside, under cover as it were; it will proceed along the path it has been really pursuing *in the medium* all the time, and make its exit at  $Y$ . In this latter case, of different motion of the medium inside and outside the telescope, the apparent direction, such as  $Y A$ , is not the true direction of the source. *The ray is in fact bent where it enters the differently-moving medium* (as shown in Fig. 4).



Ray through a Moving Stratum.

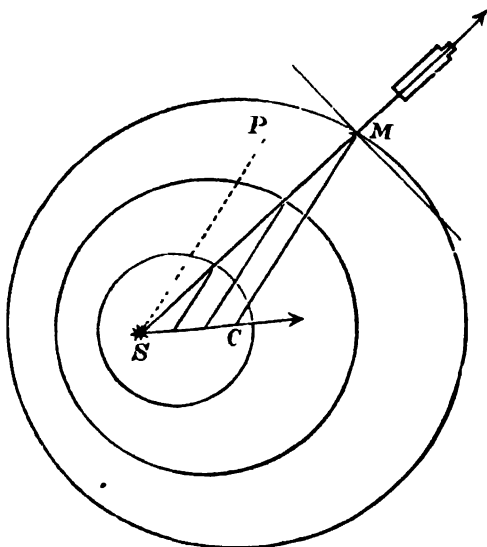
A slower moving stratum bends an oblique ray, slanting with the motion, in the same direction as if it were a denser medium.

A quicker stratum bends it oppositely. If a medium is both denser and quicker moving, it is possible for the two bendings to be equal and opposite, and thus for a ray to go on straight. Parenthetically I may say that this is precisely what happens, on Fresnel's theory, down the axis of a water-filled telescope exposed to the general terrestrial ether drift.

In a moving medium waves do not advance in their normal direction, they advance slantways. The direction of their advance is properly called a ray. The ray does not coincide with the wave-normal in a moving medium.

All this is well-shown in Fig. 5.

FIG. 5.



Successive Wave Fronts in Moving Medium.

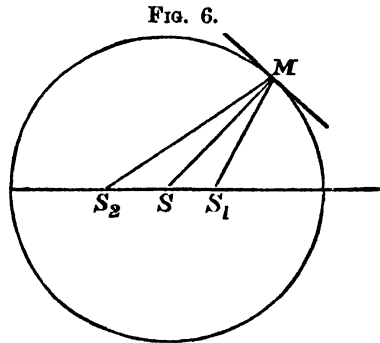
S is a stationary source emitting successive waves, which drift as spheres to the right. The wave which has reached M has its centre at C, and CM is its normal; but the disturbance, M, has really travelled along SM, which is therefore the ray. It has advanced as a wave from S to P, and has drifted from P to M. Disturbances subsequently emitted are found along the ray, precisely as in Fig. 2. A stationary telescope receiving the light will point straight at S. A mirror, M, intended to reflect the light straight back must be set normal to the ray, not tangential to the wave front.

The diagram also equally represents the case of a moving source in a stationary medium. The source, starting at C, has moved to S, emitting waves as it went, which waves as emitted spread out as simple spheres from the then position of source as centre. Wave-normal and ray now coincide: SM is not a ray, but only the locus of

successive disturbances. A stationary telescope will look not at  $S$ , but along  $MC$  to a point where the source was when it emitted the wave  $M$ ; a moving telescope, if moving at same rate as source, will look at  $S$ . Hence  $SM$  is sometimes called the *apparent ray*. The angle  $SMC$  is the aberration angle.

Fig. 6 shows normal reflection for the case of a moving source. The mirror  $M$  reflects light received from  $S_1$  to a point  $S_2$ , just in time to catch the source there.

Parenthetically I may say that the time taken on the double journey,  $S_1MS_2$ , is not quite the same as the double journey  $SM S$ , when all is stationary, and that this is the principle of Michelson's great experiment.



Normal reflection.

For the rest of the lecture I am going to call the medium which conveys light, "ether" simply. Every one knows that ether is the light conveying medium, however little else they know about the properties of that tremendously important material.

We have arrived at this: that a uniform ether stream all through space causes no aberration, no error in fixing direction. It blows the waves along, but it does not disturb the line of vision.

Stellar aberration exists, but it depends on motion of observer, and on motion of observer only. Etherial motion has no effect upon it, and when the observer is stationary with respect to object, as he is when using a terrestrial telescope, there is no aberration at all.

Surveying operations are not rendered the least inaccurate by the existence of a universal ethereal drift; and they therefore afford no means of detecting it.

But observe that everything depends on the ethereal motion being uniform everywhere, inside as well as outside the telescope, and along the whole path of the ray. If stationary anywhere it must be stationary altogether. There must be no boundary between stationary and moving ether, no plane of slip, no quicker motion even in some regions than in others. For (referring back to the remarks preceding Fig. 4) if the ether in receiver is stagnant while outside it is moving, a wave which has advanced and drifted as far as the telescope will cease to drift as soon as it gets inside, but will advance simply along the wave normal; and in general at the boundary of any such change of motion a ray will be bent, and an observer looking along the ray will see the source not in its true position, not even in the apparent position appropriate to his own motion, but lagging behind that position.

Such an aberration as this, a lag or negative aberration, has never yet been observed; but if there is any slip between layers of ether,

if the earth carries any ether with it, or if the ether, being in motion at all, is not equally in motion everywhere throughout every transparent substance, then such a lag or negative aberration must occur: in precise proportion to the amount of the carriage of ether by moving bodies.

On the other hand, if the ether behaves as a perfectly frictionless inviscid fluid, or if for any other reason there is no rub between it and moving matter, so that the earth carries no ether with it at all, then all rays will be straight, aberration will have its simple and well-known value, and we shall be living in a virtual ether stream of 19 miles a second, by reason of the orbital motion of the earth.

It may be difficult to imagine that a great mass like the earth can rush at this tremendous pace through a medium without disturbing it. It is not possible for an ordinary sphere in an ordinary fluid. At the surface of such a sphere there is a viscous drag, and a spinning motion diffuses out thence through the fluid so that the energy of the moving body is gradually dissipated. The persistence of terrestrial and planetary motions shows that ethereal viscosity, if existent, is small; or at least that the amount of energy thus got rid of is a very small fraction of the whole. But there is nothing to show that an appreciable layer of ether may not adhere to the earth and travel with it, even though the force acting on it be but small.

This, then, is the question before us:—

*Does the earth drag some ether with it? or does it slip through the ether with perfect freedom?* (Never mind the earth's atmosphere; the part it plays is not important.)

In other words, is the ether wholly or partially stagnant near the earth, or is it streaming past us with the opposite of the full terrestrial velocity of nineteen miles a second? Surely if we are living in an ether stream of this rapidity we ought to be able to detect some evidence of its existence.\*

It is not so easy a thing to detect as you would imagine. We have seen that it produces no deviation or error in direction. Neither does it cause any change of colour or Doppler effect; that is, no shift of lines in spectrum. No steady wind can affect pitch, simply because it cannot blow waves to your ear more quickly than they are emitted. It hurries them along, but it lengthens them in the same proportion, and the result is that they arrive at the proper frequency. The precise effects of motion on pitch are summarised in the following table:—

#### *Changes of Frequency due to Motion.*

Source approaching shortens waves.

Receiver approaching alters relative velocity.

Medium flowing alters both wave-length and velocity in exactly compensatory manner.

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\* The word "stationary" is ambiguous. I propose to use "stagnant," as meaning stationary with respect to the earth, i. e. as opposed to stationary in space.

What other phenomena may possibly result from motion? Here is a list:—

*Phenomena resulting from Motion.*

(1) Change or apparent change in direction; observed by telescope, and called aberration.

(2) Change or apparent change in frequency; observed by spectroscopes, and called Doppler effect.

(3) Change or apparent change in time of journey; observed by lag of phase or shift of interference fringes.

(4) Change or apparent change in intensity; observed by energy received by thermopile.

Motion of either source or receiver can alter frequency, motion of receiver can alter apparent direction, motion of the medium can do neither; but surely it can hurry a wave so as to make it arrive out of phase with another wave arriving by a different path, and thus produce or modify interference effects.

Or again, it may carry the waves down stream more plentifully than up stream, and thus act on a pair of thermopiles, arranged fore and aft at equal distances from a source, with unequal intensity.

And again, perhaps the laws of reflection and refraction in a moving medium are not the same as they are if it be at rest. Then, moreover, there is double refraction, colours of thin plates and thick plates, polarisation angle, rotation of the plane of polarisation; all sorts of optical phenomena.

It may be, perhaps, that in empty space the effect of an ether drift is difficult to detect, but will not the presence of dense matter make it easier?

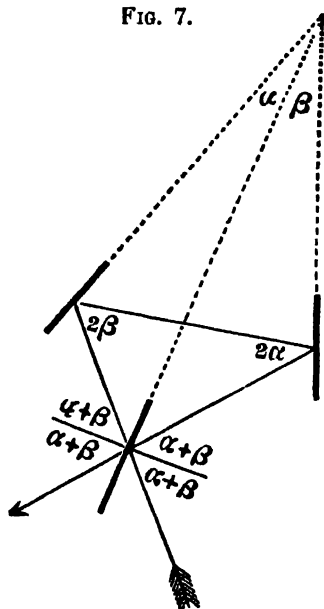
Consider No. 3 of the phenomena tabulated above. I expect that every one here understands interference, but I may just briefly say that two similar sets of waves "interfere" whenever and wherever the crests of one set coincide with and obliterate the troughs of the other set. Light advances in any given direction when crests in that direction are able to remain crests, and troughs to remain troughs. But if we contrive to split a beam of light into two halves, to send them round by different paths, and make them meet again, there is no guarantee that crest will meet crest and trough trough; it may be just the other way in some places, and wherever that opposition of phase occurs there there will be local obliteration or "interference." Two reunited half-beams of light may thus produce local stripes of darkness, and these stripes are called interference bands.

If I can I will produce actual interference of light on the screen, but the experiment is a difficult one to make visible at a distance, partly because the stripes or bands of darkness are usually very narrow. I have not seen it attempted before. [Very visible bands

were formed on screen by three mirrors, one of them semi-transparent, as in Fig. 7.]

Now a most interesting and important, and I think now well-known experiment of Fizeau proves quite simply and definitely

FIG. 7.



Plan of Interference Kaleidoscope.

that if light be sent along a stream of water, travelling inside the water as a transparent medium, it will go quicker with the current than against it. You may say that is only natural; a wind helps sound along one way and retards it the opposite way. Yes, but then sound travels in air, and wind is a bodily transfer of air, hence, of course, it gives the sound a ride; whereas light does not really travel in water, but always in ether. It is by no means obvious whether a stream of water can help or hinder it. Experiment decides, however, and answers in the affirmative. It helps it along with just about half the speed of the water; not with the whole speed, which is curious and important, and really means that the moving water has no effect whatever on the ether of space, though it would take too long to make clear how this comes about. Suffice for present purposes the fact that the velocity of light inside moving water, and there-

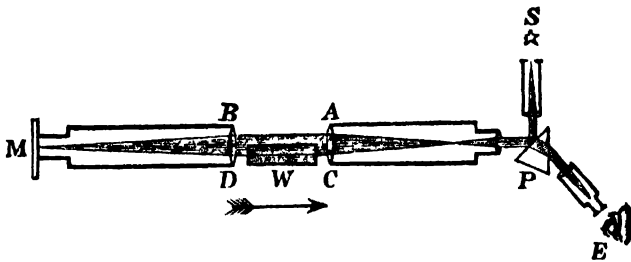
fore presumably inside all transparent matter, is altered by motion of that matter.

Does not this fact afford an easy way of detecting a motion of the earth through the ether? Here on the table is water travelling along nineteen miles a second. Send a beam of light through it one way and it will be hurried; its velocity, instead of being 140,000 miles a second, will be 140,009 miles. Send a beam of light the other way, and its velocity will be 139,991; just as much less. Bring these two beams together; surely some of their wave-lengths will interfere. M. Hoek, Astronomer at Utrecht, tried the experiment in this very form; here is a diagram of his apparatus (Fig. 8). Babinet had tried another form of the experiment previously. Hoek expected to see interference bands, from the two half-beams which had traversed the water, one in the direction of the earth's motion and the other against it. But no interference bands were seen. The experiment gave a negative result.

An experiment, however, in which nothing is seen is never a very satisfactory form of a negative experiment; it is, as Macart calls it,

"doubly negative," and we require some guarantee that the condition was right for seeing what might really have been in some sort there. Hence Mascart and Jamin's modification of the experiment is prefer-

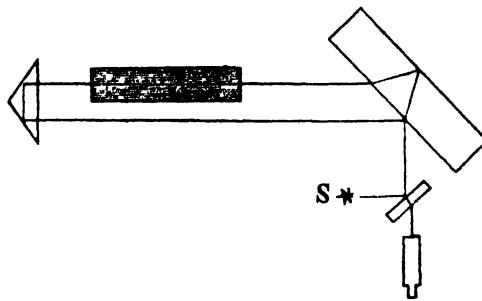
FIG. 8.



Hoek's arrangement.

able (Fig. 9). The thing now looked for is a shift of already existing interference bands, when the above apparatus is turned so as to have different aspects with respect to the earth's motion ; but no shift was seen.

FIG. 9.



Arrangement of Mascart and Jamin.

Interference methods all fail to display any trace of relative motion between earth and ether.

Try other phenomena then. Try refraction. The index of refraction of glass is known to depend on the ratio of the speed of light outside, to the speed inside, the glass. If then the ether be streaming through glass, the velocity of light will be different inside it according as it travels with the stream or against it, and so the index of refraction will be different. Arago was the first to try this experiment by placing an achromatic prism in front of a telescope on a mural circle, and observing the deviation it produced on stars.

Observe that it was an *achromatic* prism, treating all wave-lengths

alike; he looked at the *deviated* image of a star, not at its *dispersed* image or spectrum, else he might have detected the change-of-frequency-effect due to motion of source or receiver first actually seen by Dr. Huggins. I do not think he would have seen it, because I do not suppose his arrangements were delicate enough for that very small effect; but there is no error in the conception of his experiment, as Prof. Mascart has inadvertently suggested there was.

Then Maxwell repeated the attempt in a much more powerful manner, a method which could have detected a very minute effect indeed, and Mascart has also repeated it in a simple form. All are absolutely negative.

Well, what about aberration? If one looks through a moving stratum, say a spinning glass disk, there ought to be a shift caused by the motion (see Fig. 4). The experiment has not been tried, but I entertain no doubt about its result, though a high speed and considerable thickness of glass or other medium is necessary to produce even a microscopic apparent displacement of objects seen through it.

But the speed of the earth is available, and the whole length of a telescope tube may be filled with water; surely that is enough to displace rays of light appreciably.

Sir Geo. Airy tried it at Greenwich on a star, with an appropriate zenith-sector full of water. Stars were seen through the water-telescope precisely as through an air telescope. A negative result again.

Stellar observations, however, are unnecessarily difficult. Fresnel had said that a terrestrial source of light would do just as well. He had also (being a man of exceeding genius) predicted that nothing would happen. Hook has now tried it in a perfect manner and nothing did happen.

Since then Prof. Mascart with great pertinacity has attacked the phenomena of thick plates, Newton's rings, double refraction, and the rotatory phenomenon of quartz; but he has found absolutely nothing attributable to a stream of ether past the earth.

The only positive result ever supposed to be attained was in a very difficult polarisation observation by Fizeau in 1859. As this has not yet been repeated, it is safest at present to ignore it, though by no means to forget that it wants repeating.

Fizeau also suggested, but did not attempt, what seems an easier experiment, with fore and aft thermopiles and a source between them, to observe the drift of a medium by its convection of energy; but arguments based on the law of exchanges\* tend to show, and do show as I think, that a probable alteration of radiating power due to motion through a medium would just compensate the effect otherwise to be expected.

We may summarise most of these statements as follows :—

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\* Lord Rayleigh, 'Nature,' March 25, 1892.



*Summary.*

Source alone moving produces .. .. .	{	A real and apparent change of wave-length.
		A real but not apparent error in direction.
		No lag of phase or change of intensity, except that appropriate to altered wave-length.
Medium alone moving, or source and receiver moving together, produces .. .. .	{	No change of frequency.
		No error in direction.
		A real lag of phase, but undetectable without control over the medium.
Receiver alone moving produces .. .. .	{	A change of intensity corresponding to different distance, but compensated by change of radiating power.
		An apparent change of wave-length.
		An apparent error in direction.
		No change of phase or of intensity, except that appropriate to different virtual velocity of light.

I may say, then, that not a single optical phenomenon is able to show the existence of an ether stream near the earth. All optics go on precisely as if the ether were stagnant with respect to the earth.

Well then perhaps it *is* stagnant. The experiments I have quoted do not prove that it is so. They are equally consistent with its perfect freedom and with its absolute stagnation, though they are not consistent with any intermediate position. Certainly, if the ether were stagnant nothing could be simpler than their explanation.

The only phenomena then difficult to explain would be those depending on light coming from distant regions through all the layers of more or less dragged ether. The theory of astronomical aberration would be seriously complicated; in its present form it would be upset. But it is never wise to control facts by a theory; it is better to invent some experiment that will give a different result in stagnant and in free ether. None of those experiments so far described are really discriminative. They are, as I say, consistent with either hypothesis, though not very obviously so.

Mr. Michelson, however, of Harvard, U.S., has invented a plan that will discriminate; and, what is much more remarkable, he has carried it out.

That it is an exceptionally difficult experiment you will realise when I say that the experiment will fail altogether unless one part in 400 millions can be clearly detected.

Mr. Michelson reckons that by his latest arrangement he could see 1 in 4000 millions if it existed (which is equivalent to detecting an error of  $\frac{1}{1000}$  of an inch in a length of 40 miles); but he saw nothing. Everything behaved precisely as if the ether was stagnant; as if the earth carried with it all the ether in its immediate neighbourhood. And that is his conclusion. If he can repeat it and get a different result on the top of a mountain, that conclusion may be considered established. At present it must be regarded as tentative.

I have not time to go into the details of his experiment (it is described in 'Phil. Mag.,' 1887), but I may say that it depends on

no doubtful properties of transparent substances, but on the straightforward fundamental principle underlying all such simple facts as that—It takes longer to row a certain distance and back up and down stream than it does to row the same distance in still water; or that it takes longer to run up and down a hill than to run the same distance laid out flat; or that it costs more to buy a certain number of oranges at three a penny and an equal number at two a penny than it does to buy the whole lot at five for twopence.

Hence, although there may be some way of getting round Mr. Michelson's experiment, there is no obvious way; and I conjecture that if the true conclusion be not that the ether near the earth is stagnant it will lead to some other important and unknown fact.

The balance of evidence at this stage seems to incline in the sense that the earth carries the neighbouring ether with it.

But now put the question another way. *Can matter carry neighbouring ether with it when it moves?* Abandon the earth altogether; its motion is very quick, but too uncontrollable, and it always gives negative results. Take a lump of matter that you can deal with, and see if it pulls any ether along.

That is the experiment I set myself to perform, and which in the course of the last year, I have performed.

I take a steel disk, or rather a couple of steel disks clamped together with a space between. I mount it on a vertical axis and spin it like a teetotum as fast as it will stand without flying to pieces. Then I take a parallel beam of light, split it into two by a semi-transparent mirror (Michelson's method), a piece of glass silvered so thinly that it lets half the light through and reflects the other half; and I send the two halves of this split beam round and round in opposite directions in the space between the disks. They may thus travel a distance of 20 or 30 or 40 feet. Ultimately they are allowed to meet and enter a telescope. If they have gone quite identical distances they need not interfere, but usually the distances will differ by a hundred-thousandth of an inch or so, which is quite enough to bring about interference.

The mirrors which reflect the light round and round between the disks are shown in Fig. 10. If they form an accurate square the last two images will coincide, but if the mirrors are the least inclined to one another at any unaliquot part of  $360^\circ$  the last image splits into two, as in the kaleidoscope is well known, and the interference bands may be regarded as resulting from those two sources. The central white band bisects normally the distance between them, and their amount of separation determines the width of the bands. There are many interesting optical details here, but I shall not go into them.

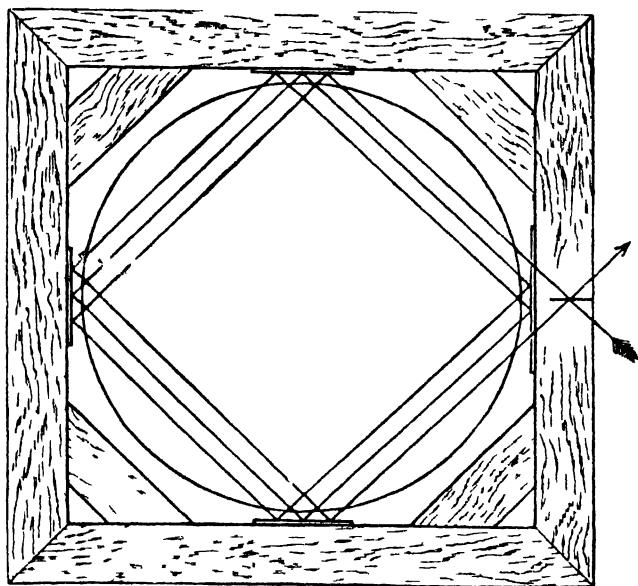
The thing to observe is whether the motion of the disks is able to replace a bright band by a dark one, or *vice versa*. If it does, it means that one of the half beams, viz. that which is travelling in the same direction as the disks, is helped on a trifle, equivalent to a shortening of journey by some quarter millionth of an inch or so in

the whole length of 30 feet; while the other half beam, viz. that travelling against the motion of the disks, is retarded, or its path virtually lengthened, by the same amount.

If this acceleration and retardation actually occurs, waves which did not interfere on meeting, before the disks moved, will interfere now, for one will arrive at the common goal half a length behind the other.

Now a gradual change of bright space to dark, and *vice versâ*, shows itself, to an observer looking at the bands, as a gradual change of position of the bright stripes, or a shift of the bands. A shift of

FIG 10.



Plan of Steel Disks, one yard in diameter, and Optical Frame.

the bands, and especially of the middle white band, which is much more stable than the others, is what we look for.

At first I saw plenty of shift. In the first experiment the bands sailed across the field as the disks got up speed until the crosswire had traversed a band and a half. The conditions were such that had the ether whirled at the full speed of the disks I should have seen a shift of three bands. It looked very much as if the light was helped along at half the speed of the moving matter, just as it is inside water.

On stopping the disks the bands returned to their old position. On starting them again in the opposite direction, the bands ought to have shifted the other way too; but they did not; they went the same way as before.

The shift was therefore wholly spurious; it was caused by the centrifugal force of the blast of air thrown off from the moving disks. The mirrors and frame had to be protected from this. Many other

small changes had to be made, and gradually the spurious shifts have been reduced and reduced, largely by the skill and patience of my assistant, Mr. Davies, until now there is barely a trace of them.

But the experiment is not an easy one. Not only does the blast exert pressure, but at high speeds the churning of the air makes it quite hot. Moreover, the tremor of the whirling machine, in which some four or five horse-power is sometimes being expended, is but too liable to communicate itself to the optical part of the apparatus. Of course elaborate precautions are taken against this. Although the two parts, the mechanical and the optical, are so close together, their supports are entirely independent. But they have to rest on the same earth, and hence communicated tremors are not absent. They are the cause of all the slight residual trouble.

The method of observation now consists in setting a wire of the micrometer accurately in the centre of the middle band, while another wire is usually set on the first band to the left. Then the micrometer heads are read, and the setting repeated once or twice to see how closely and dependably they can be set in the same position. Then we begin to spin the disks, and when they are going at some high speed, measured by a siren note and in other ways, the micrometer wires are reset and read—reset several times and read each time. Then the disks are stopped and more readings are taken. Then their motion is reversed, the wires set and read again; and finally the motion is once more stopped and another set of readings taken. By this means the absolute shift of middle band and its relative interpretation in terms of wave-length are simultaneously obtained; for the distance from the one wire to the other, which is often two revolutions of a micrometer head, represents a whole wave-length shift.

In the best experiments I do still often see something like a fiftieth of a band shift, but it is caused by residual spurious causes, for it repeats itself with sufficient accuracy in the same direction when the disks are spun the other way round.

Of real reversible shift, due to motion of the ether, I see nothing. I do not believe the ether moves. It does not move at a five-hundredth part of the speed of the steel disks. I hope to go further, but my conclusion so far is that such things as circular-saws, flywheels, railway trains, and all ordinary masses of matter do not appreciably carry the ether with them. Their motion does not seem to disturb it in the least.

The presumption is that the same is true for the earth; but the earth is a big body, it is conceivable that so great a mass may be able to act when a small mass would fail. I would not like to be too sure about the ether. What I do feel already pretty sure of is that if moving matter disturbs ether in its neighbourhood at all, it does, so by some minute action, comparable in amount perhaps to gravitation, and possibly by means of the same property as that to which gravitation is due—not by anything that can fairly be likened to ethereal viscosity.

[O. L.]

Friday, June 3, 1892.

SIR FREDERICK ABEL, K.C.B. D.C.L. F.R.S. Vice-President,  
in the Chair.

LUDWIG MOND, Esq. F.R.S. M.R.I.

*Metallic Carbonyls.*

JUSTUS LIEBIG, perhaps the most prophetic mind among modern men of science, wrote in the year 1834, in the 'Annalen der Pharmacie,' "I have previously announced that carbonic oxide may be considered as a radical, of which carbonic acid and oxalic acid are the oxides, and phosgene gas is the chloride. The further pursuit of this idea has led me to the most singular and the most remarkable results."

Liebig has not told us what these results were, and it has taken many years before the progress of chemical research has revealed to us what may at that early date have been before Liebig's vision. I will to-night bring before you some important discoveries made only within the last few years by following up Liebig's idea.

Carbonic oxide, composed of one atom of carbon and one atom of oxygen, is a colourless gas, without taste or smell, which I have here in this jar. It burns in air with a blue flame. When it acts as a radical, combining with other bodies, we term it carbonyl, and its compounds with other elements or radicals are termed carbonyls. Liebig defined a radical as a compound having the characteristics of a simple body, which would combine with, replace, and be replaced by simple bodies. In more modern times a radical has been defined as an unsaturated body. I am of course speaking of chemical radicals. If we look at it from the modern point of view, carbonyl should be the very model of a radical, because only half of the four valencies of carbon are satiated, the other two remaining free. Carbonic oxide should even be a most violent radical because amongst all organic radicals it is the only one we know to exist in the atomic or free state. All the other organic radicals, even such typical ones as cyanogen and acetylene, are known to us as molecules composed of two atoms of the radical, so that the cyanogen gas and acetylene gas we know should more properly be called di-cyanogen and di-acetylene; they consist of two atoms of the radical cyanogen or of the radical acetylene, the free valencies or combining powers of which satiate or neutralise each other. On the other hand, carbonic oxide gas, as I stated before, makes the sole exception. Its molecule contains only one atom of carbonyl moving about with its free valencies unfettered by a second atom. For all that, carbonic oxide is by no means a violent body, but the very reverse, and instead of being

ready to attack with its two free valencies anything coming in its way, until very recently we only knew it to interact and to combine with substances possessing themselves extreme attacking powers, such as chlorine and potassium. Although Liebig had so long ago proclaimed it as a radical, the chemical world was startled when, two years ago, I announced in a paper I communicated to the Chemical Society in conjunction with Drs. Langer and Quincke, that carbonic oxide combines at ordinary temperature with so inactive an element as nickel, and forms a well-defined compound of very peculiar properties.

The fact that carbonic oxide does not possess the chemical activity one would suppose in a radical composed of single atoms may, I believe, be explained by assuming that the two valencies of carbon which are not combined with oxygen do satiate or neutralise each other. Everybody admits that the valencies of two different carbon atoms, which are all considered of equal value, can neutralise each other. I see, therefore, no reason to question the possibility of two valencies of the same carbon atom neutralising each other. On this assumption carbonic oxide may be looked upon as a self satisfied body—one which keeps in check its free affinities within itself. I have tried to explain this by the graphic formula in this diagram.



You have here (see diagram on next page) the typical carbon radicals containing one atom of that element, acetylene, methylene, methyl, cyanogen, and carbonyl. In the second column you have these substances as they are known to us in the free state. You see the carbonyl is the only one which exists in the free state as a single atom, while all the others only exist as molecules, composed of two atoms the free valencies of which neutralise each other. The carbonyl I have represented in the last formula, with the two valencies not combined with oxygen neutralising each other, so that in this way it also becomes a satiated body.

The paper published by Liebig in 1834, from which I have already quoted, was entitled "On the Action of Carbonic Oxide on Potassium." In it Liebig fully described the preparation and properties of the first metallic carbonyl known—a compound of potassium and carbonic oxide. Liebig obtained this compound by the direct action of carbonic oxide upon potassium at a temperature of 80° C., and proved it to be identical with a substance which had been previously obtained as a very disagreeable bye-product of the manufacture of potassium from potash and carbon by Brunner's method. It forms a grey powder which is not volatile, and which on treatment with water yields a red solution, gradually turning yellow in contact with air and from which on evaporation a yellow salt is obtained called potassium croconate, on account of its colour. Liebig showed this

## TYPICAL CARBON RADICALS.

I.		II. (Known in the free state.)	
Acetylene	$\begin{array}{c} \text{---} \text{C} \text{---} \text{H} \\ \text{---} \end{array}$	Di-acetylene	$\text{H} \text{---} \begin{array}{c} \text{---} \text{C} \text{---} \text{C} \text{---} \\ \text{---} \end{array} \text{H} \quad \text{C}_2\text{H}_2$
Methylene	$\begin{array}{c} \text{---} \text{C} \text{---} \text{H} \\ \text{---} \end{array}$	Di-methylene (Olefiant gas)	$\begin{array}{c} \text{H} \text{---} \text{C} \text{---} \text{C} \text{---} \text{H} \\ \text{H} \text{---} \end{array} \quad \text{C}_2\text{H}_4$
Methyl	$\begin{array}{c} \text{---} \text{C} \text{---} \text{H} \\ \text{---} \end{array}$	Di-methyl	$\begin{array}{c} \text{H} \text{---} \text{C} \text{---} \text{C} \text{---} \text{H} \\ \text{H} \text{---} \end{array} \quad \text{C}_2\text{H}_6$
Cyanogen	$\begin{array}{c} \text{---} \text{O} \text{---} \text{N} \\ \text{---} \end{array}$	Di-cyanogen	$\text{N} \text{---} \begin{array}{c} \text{---} \text{C} \text{---} \text{C} \text{---} \\ \text{---} \end{array} \text{N} \quad \text{C}_2\text{N}_2$
Carbonyl	$\begin{array}{c} \text{---} \text{C} \text{---} \text{O} \\ \text{---} \end{array}$	Carbonic oxide	$\begin{array}{c} \text{---} \text{C} \text{---} \text{O} \\ \text{---} \end{array} \quad \text{CO}$

salt to consist of two atoms of potassium, five of carbon, and five of oxygen, and not to contain any hydrogen, as had previously been supposed.

Since the publication of Liebig's paper, potassium carbonyl has been studied by numerous investigators, amongst whom Sir Benjamin Brodie deserves particular mention; but it has been reserved to Nietzki and Benkiser to determine finally in the year 1885, by a series of brilliant investigations, its exact constitution and its place in the edifice of chemistry. They have proved that it has the formula  $\text{K}_6\text{C}_5\text{O}_5$ ; that the six carbons in this compound are linked together in the form of a benzole ring; that, in fact, the compound is hexhydroxylbenzole, in which all the hydrogen is replaced by potassium. By simple treatment with an acid it can be converted into the hexhydroxylbenzole, and from this substance it is possible to produce, by a series of reactions well known to organic chemists, the whole wide range of the benzole compounds. The body which Liebig obtained by the direct action of carbonic oxide on potassium has thus enabled us to prepare synthetically in a very simple way from purely inorganic substances—to wit, from potash and carbon, or if we like even from potash and iron—the whole series of those most

important and interesting compounds called aromatic compounds, including all the coal-tar colours, which have furnished us with an undreamt of variety of innumerable hues and shades of colour, as well as many new substances of great value to suffering humanity as medicines. Surely a startling result, which alone would have fully justified Liebig's prediction of 1834!

Speaking of coal-tar colours, everybody will be reminded of the great loss the scientific world has recently sustained by the death of August Wilhelm Hofmann, their first discoverer, Liebig's greatest pupil. Hofmann will ever be remembered in this Institution, where he so often delighted the audience by his lucid lectures, and in whose welfare he took the greatest interest, of which he gave us a fresh proof only last year, in the charming letter he wrote on the occasion of his election as an Honorary Member.

Looking back upon the wonderful outcome of Liebig's idea I have referred to, it seems surprising indeed that others should not have followed up his work by attempting to obtain other metallic carbonyls.

A very few experiments were made with other alkaline metals. Sodium, otherwise resembling potassium so closely, has been shown not to combine with carbonic oxide; lithium and calcium are stated to behave similar to potassium. But metals of other groups received little or no attention. The very important rôle which carbonic oxide plays in the manufacture of iron did lead to a number of metallurgists (among whom Sir Lowthian Bell and Dr. Alder Wright are the most prominent) to study its action upon metallic iron and other heavy metals, including nickel and cobalt at high temperatures. They proved that these metals have the property to split up carbonic oxides into carbon and carbonic acid at a low red heat, a result of great importance, which threw a new light upon the chemistry of the blast furnace. None of these investigators, however, turned their attention to obtaining compounds of these metals with carbonic oxide, and, owing to the high temperature and the other conditions under which they worked, the existence of such compounds could not come under their observation. In order to obtain these compounds, very special conditions must be observed, which are fully described in the papers I have published during the last two years in conjunction with Dr. Langer and Dr. Quincke.

The metals must be prepared with great care, so as to obtain them in an extremely fine state of division, and must be treated with carbonic oxide at a *low temperature*. The best results are obtained when the oxalate of the metal is heated in a current of hydrogen at the lowest temperature at which its reduction to the metallic state is possible. I have in the tube before me metallic nickel prepared in this way, and over which a slow current of carbonic oxide is now passing. The carbonic oxide before entering the tube burns, as you see, with a blue non-luminous flame. After passing over the nickel it burns with a highly luminous flame, which is due to the separation of metallic nickel from the nickel carbonyl formed in the tube, which is



heated to incandescence in the flame. If we pass the gas through a freezing mixture, you will observe that a colourless liquid is condensed, of which I have a larger quantity standing in this tube. In passing the gas issuing from our tube through a glass tube heated to about  $200^{\circ}\text{C}$ ., we obtain a metallic mirror of pure nickel, because at this temperature the nickel carbonyl is again completely resolved into its components, nickel and carbonic oxide. We will by-and-by show you that this mirror consists of pure nickel. This liquid is pure nickel carbonyl, and has the formula  $\text{Ni}(\text{CO})_4$ . It has a specific gravity at ordinary temperature of 1.3185, and boils under atmospheric pressure at the low temperature of  $43^{\circ}\text{C}$ .

It has a very high vapour tension at ordinary temperature and possesses a very high rate of expansion. If cooled to  $-25^{\circ}\text{C}$ . it solidifies, forming needle-shaped crystals. A mixture of the vapour with air explodes readily, sometimes at ordinary temperature, but without violence, as we will show you. The liquid itself in the pure state does not explode, but decomposes into its constituents when heated sufficiently. The vapour of nickel carbonyl possesses a characteristic odour and is poisonous, but not more so than carbonic oxide gas. Prof. McKendrick has studied the physiological action of this liquid, and has found that, when injected subcutaneously in extremely small doses in rabbits, it produces an extraordinary reduction of temperature, in some cases as much as  $12^{\circ}$ .

The liquid can be completely distilled without decomposition, but from its solution in liquids of a higher boiling point it cannot be obtained by rectification. On heating such a solution the compound is decomposed, nickel being separated in the liquid, while carbonic oxide gas escapes. I will try to demonstrate this by an experiment.

We have here a solution of the substance in heavy petroleum oil, which you will, in a few minutes, see turn completely black on heating by the separation of nickel, while a gas escapes which is pure carbonic oxide.

In a similar way, when the nickel carbonyl is attacked by oxidising agents, such as nitric acid, chlorine, or bromine, it is readily broken up, nickel salts being formed, and carbonic oxide being liberated. Sulphur acts in a similar way. Metals, even potassium, alkalis, and acids, which have no oxidizing power, will not act upon the liquid at all, nor do the salts of other metals react upon it. The substance behaves therefore, chemically, in an entirely different manner from potassium carbonyl, and does not lead, as the other does, by easy methods to complicated organic compounds. It does not show any one of the reactions which are so characteristic for organic bodies containing carbonyl, such as the Ketones and Quinones; and we have not been able, in spite of very numerous experiments, either to substitute the carbonic oxide in this compound by other bivalent groups, or to introduce the carbonic oxide by means of this compound into organic substances.

By exposing the liquid to atmospheric air a precipitate of carbo-

nate of nickel is slowly formed of varying composition, which is yellowish-white if perfectly dry air is used, and varies from a light green to a brownish colour if more or less moisture is present.

We have found all these precipitates to dissolve easily and completely in dilute acid, with evolution of carbonic acid, leaving ordinary nickel salts behind, and can therefore not agree with the view propounded by Professor Berthelot in a communication to the French Academy of Science, that these precipitates contain a compound of nickel with carbon and oxygen, comparable to the so-called oxides of organo-metallic compounds. In the same paper Professor Berthelot has described a beautiful reaction of nickel carbonyl with nitric oxide, which Dr. Langer will now show you. You will notice the intense blue coloration which the liquid solution of nickel carbonyl in alcohol assumes by passing the nitric oxide through it. Professor Berthelot has reserved to himself the study of this body, but has so far not published anything further about it.

The chemical properties of the compound I have just described to you are without parallel; we do not know a single substance of similar properties. It became, therefore, of special interest to study the physical properties of the compound.

Professor Quincke, of Heidelberg, has kindly determined its magnetic properties, and found that it possesses in a high degree the property discovered by Faraday, and called by him dia-magnetism, which is the more remarkable, as all the other nickel compounds are para-magnetic. He also found that it is an almost perfect non-conductor of electricity, in this respect differing from all other nickel compounds.

The absorption spectrum, and also the flame spectrum of our compound are at present under investigation by those indefatigable spectroscopists, Professors Dewar and Liveing, by whose kindness I am enabled to bring before you, in advance of a paper they are sending to the Royal Society, some of the interesting results they have obtained. We have here a photograph of the absorption spectrum, obtained by means of a hollow prism through quartz plates filled with nickel carbonyl, through which the spark spectrum of iron is passed, which is photographed on the same plate. You see that the whole of the ultra-violet rays of the iron spectrum have disappeared, being completely absorbed by the nickel carbonyl, which is thus quite opaque for all the rays beyond the wave-lengths 3820.

The spectrum of the highly luminous flame of nickel carbonyl, which I have shown you before, is quite continuous; but if the nickel carbonyl is diluted with hydrogen, and the mixture burnt by means of oxygen, the gases burn with a bright yellowish-green flame without visible smoke; and the spectrum of this flame shows in its visible part, on a background of a continuous spectrum, a large number of bands, brightest in the green, but extending on the red side beyond the red line of lithium, and on the violet side well into the blue. These bands cannot be seen on the photograph which I will now

show you, the visible part of the spectrum appearing continuous, but beyond the visible part, the photograph shows a large number—over fifty—of well-defined lines in the ultra-violet. I will show you these lines in another photograph taken with greater dispersion, and on which have also been photographed the spark spectrum of nickel. You will see that all these lines correspond absolutely to lines appertaining to the spark spectrum; in fact the greater part of the lines in the spark spectrum are also shown in this flame spectrum. We have here another and very striking example of the fact discovered on the same day by Professors Dewar and Liveing and by Dr. Huggins, that the spectrum of luminous flames is not always continuous throughout its whole range, a fact which has at one time been much debated and discussed.

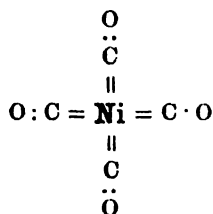
One of the most remarkable discoveries made within the precincts of this Institution by that illustrious man whose centenary we celebrated last year was that of the connection between magnetism and light, which manifests itself when a beam of polarised light is sent through a substance while it is subjected to a strong magnetic field, under whose influence the beam of light is rotated through a certain angle. Dr. W. H. Perkin has prosecuted this discovery of Faraday's by a long series of most elaborate researches, and has established the fact that this power of magnetic rotation of various bodies has a definite relation to their chemical constitution, and enables us to gain a better insight into the structure of chemical compounds. Dr. Perkin has been good enough to investigate the power of magnetic rotation of the nickel carbonyl, and has found it quite as unusual as its chemical properties, and to be, with the sole exception of phosphorus, greater than that of any other substance he has yet examined.

The power of different bodies of refracting and dispersing a ray of light has been shown by the beautiful and elaborate researches undertaken many years ago by Dr. Gladstone—who has given an account of them in this theatre in 1875, and who has since continued them with indefatigable zeal—to throw a considerable light upon the constitution of chemical compounds.

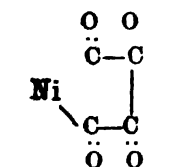
I have investigated the refractive and dispersive powers of nickel carbonyl in Rome, in conjunction with Prof. Nasini. We found that the atomic refraction of nickel in the substance is nearly two and a half times as large as it is in any other nickel compound—a difference very much greater than had ever before been observed in the atomic refraction of any element. To give you some idea how these figures are obtained, Mr. Lennox will now throw on to the screen a beam of light through a double prism, filled partly with nickel carbonyl, and partly with alcohol. You will notice that the top spectrum is turned much further to the left, showing the nickel carbonyl to possess a much greater power of refraction, and you will also notice that it is much wider than the bottom spectrum, which shows the greater dispersive power of the nickel carbonyl.

It is now generally supposed that if one element shows different

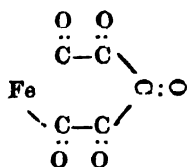
atomic refractive powers in different compounds, it enters with a larger number of valencies into the compound which shows a higher refractive power. In accordance with this view, the very much greater refractive power of the nickel in the carbonyl would find an explanation in assuming that this element, which in all its other known combinations is distinctly bivalent, exercises in the carbonyl the limit of its valency, viz. 8, assigned to it by Mendeleeff, who placed it into the eighth group in his Table of Elements. This would mean that one atom of nickel contained in the nickel carbonyl is combined directly with each of the four bivalent atoms of carbonyl, each of which would saturate two of the eight valencies of nickel, as is shown by this formula—



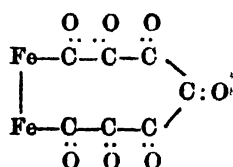
This view seems plausible, and in accordance with the chemical properties of the substance, and I should have no hesitation in accepting it if we had not, in the further pursuit of our work on metallic carbonyls, met with another substance—a liquid compound of iron with carbonic oxide—which in its properties bears so much resemblance to the nickel compound that one cannot assign to it a different constitution, whilst its composition makes the adoption of a similar structural formula next to impossible. It contains, for one equivalent of iron, five equivalents of carbonyl. To assign to it a similar constitution, one would, therefore, have to assume that iron did exercise ten valencies, or two more than any other known element, a view which very few chemists would be prepared to countenance. The atomic refraction of iron in this compound, which Dr. Gladstone has had the kindness to determine, is as unusual as that of the nickel in the nickel compound, and bears about the same ratio to the atomic refraction of iron in other compounds. We have, therefore, to find another explanation for the extraordinarily high atomic refraction of these metals in their compounds with carbon monoxide, which may possibly modify our present view on this subject. As to the structure of these compounds themselves, we are almost bound to assume that they contain the carbonyl atoms in the form of a chain, as I have represented on this diagram.



Nickel-tetra-carbonyl.



Ferro-penta-carbonyl.



Di-ferro-hepta-carbonyl.

The ferro-carbonyl is prepared in a similar manner to the nickel compound. The iron used is obtained from the oxalate at the very lowest temperature possible, and is in a high degree pyrophoric. It immediately catches fire on coming into contact with air, as I will show you.

This carbonyl forms, however, with such great difficulty, that we overlooked its existence for a long time, and great precautions have to be taken to obtain even a small quantity of it. It forms an amber-coloured liquid, of which I have a small quantity before me. It solidifies below  $-21^{\circ}\text{C}$ . to a mass of needle-shaped crystals. It distils completely at  $102^{\circ}$ . Its specific gravity is 1.466 at  $18^{\circ}\text{C}$ . On heating the vapour to  $180^{\circ}$  it is completely decomposed into iron and carbonic oxide. The iron mirrors before me have been obtained in this way. Its chemical composition is  $\text{Fe}(\text{CO})_5$ .

It is interesting that, within a short time after we had made known the existence of this body, Sir Henry Roscoe found it in carbonic oxide gas which had stood compressed in an iron cylinder for a considerable time, and expressed the opinion that the red deposit which sometimes forms in ordinary steatite gas-burners is due to the presence of this substance in ordinary illuminating gas. Its presence in compressed gas used for lime-lights has been noticed by Dr. Thorne, whose attention was called to the fact that this gas sometimes will not give a proper light because the incandescent lime becomes covered with oxide of iron.

M. Garnier, in a paper communicated to the French Academy of Science, supposes even that this gas is sometimes formed in large quantities in blast-furnaces when they are working too cold, and refers to some instances in which he found large deposits of oxide of iron in the tubes leading away the gas from these furnaces; but I find it difficult to believe that the temperature of a blast furnace could ever be sufficiently reduced as to give rise to the formation of this compound. On the other hand, it is highly probable that the formation of this compound of iron and carbonic oxide may play an important rôle in that mysterious process by which we are still making, and have been making for ages, the finest qualities of steel, called the cementation process.

The chemical behaviour of the substance towards acids and oxidising agents is exactly the same as that of the nickel compound, but to alkalis it behaves differently. The liquid dissolves without evolution of gas. After a while a greenish precipitate is formed, which contains chiefly hydrated-ferrous oxide, and the solution becomes brown. On exposure to the air it takes up oxygen; the colour changes to a dark red, whilst hydrated-ferrie oxide separates out.

We have so far not been able to obtain from this solution any compound fit for analysis, and are still engaged upon unravelling the nature of the reaction that takes place, and of the compounds that are formed.

Although the solution resembles in appearance to some extent the solutions obtained by treating potassium carbonyl with water, it does not give any of the characteristic reactions of the latter. When speaking of potassium carbonyl, I mentioned that by its treatment with water, croconate of potassium was obtained, which has the formula  $K_2C_5O_8$ .

We have transformed this by double decomposition into ferrous croconic,  $FeC_5O_8$ , a salt forming dark crystals of metallic lustre resembling iodine, which is not volatile, and dissolves readily in water, the solution giving all the well-known reaction of iron in croconic acid. You will note how entirely different the properties of this substance are from those of iron carbonyl, which I have described to you; yet, on reference to its composition, you will find that it contains exactly the same number of atoms of iron, carbon, and oxygen, as the latter. This is a very interesting case of isomerism, considering that both compounds contain only iron, carbon, and oxygen. The difference in the properties of these two bodies becomes explainable by comparing the structural formula of the two substances.

I would now call your attention to the great difference in the constitution of the potassium carbonyl and that of the nickel and ferro carbonyl. In the former the metal potassium is combined with the oxygen in the carbonyl; in the latter the metals nickel and iron are combined with the carbon of carbonyl. In the first case we have a benzole ring with its three single and three double bonds; in the second a closed chain with only single bonds. It is evident that the chemical properties of these substances must be widely different.

The ferro-penta-carbonyl remains perfectly unchanged in the dark but if it is exposed to sunlight it is transformed into a solid body of remarkably fine appearance, of gold colour and lustre, as shown by the sample in this tube.

This solid body is not volatile, but on heating it in the absence of air, iron separates out and liquid ferro-carbonyl distils over. If, however, it is heated carefully in a current of carbonic oxide it is reconverted into the ferro-penta-carbonyl and completely volatilised. We have so far found no solvent for this substance, so that we have no means as yet of obtaining it in a perfectly pure state. Several determinations of the iron in different samples of the substance have led to fairly concordant figures, which agree with the formula  $Fe_2(CO)_7$ , or di-ferro-hepta-carbonyl.

The interesting properties of the substances described have naturally led us "to try," as Lord Kelvin once put it to me so prettily, "to give wings to other heavy metals." We have tried all the well-known and a very large number of the rarer metals; but with the exception of nickel and iron we have so far been entirely unsuccessful. Even cobalt, which is so very like nickel, has not yielded the smallest trace of a carbonyl. This led me to study the question whether, by means of the action of carbonic oxide, the separa-

tion on a large scale of nickel from cobalt could not be effected, which has so far been a most complicated metallurgical operation; and subsequently I was led to investigate whether it would not be possible to use carbonic oxide to extract nickel industrially direct from its ores.

For solving these problems within the limits of the resources of a laboratory, we have devised apparatus, the principles of which are shown on this diagram. It consists of a cylinder divided into many compartments, through which the properly prepared ore is passed very slowly by means of stirrers attached to a shaft. On leaving the bottom of this cylinder, the ore passes through a transporting screw, and from this to an elevator, which returns it to the top of the cylinder, so that it passes many times through the cylinder, until all the nickel is volatilised. Into the bottom of this cylinder we pass carbonic oxide, which leaves it at the top charged with nickel carbonyl vapour, and passes through the conduits shown here into tubes set in a furnace and heated to  $200^{\circ}\text{C}$ . Here the nickel separates out from the nickel carbonyl. The carbonic oxide is regenerated and taken back to the cylinder by means of a fan, so that the same gas is made to carry fresh quantities of nickel out of the ore in the cylinder, and to deposit it in these tubes an infinite number of times.

Upon these principles Dr. Langer has constructed a complete plant on a Liliputian scale, which has been at work in my laboratory for a considerable time, and a photograph of which we will now throw on to the screen.

You see here the volatilising cylinder divided into numerous compartments, through which the ore is passing, and subjected to the action of carbonic oxide. At the bottom the ore is delivered into the transporting screw passing through a furnace (for the purpose of heating the ore to about  $350^{\circ}\text{C}$ . whereby its activity is maintained). This screw delivers into an elevator, which returns the ore to the top of the cylinder, so that the ore constantly passes at a slow rate through the cylinder again and again, until the nickel it contains has been taken out. The carbonic oxide gas, prepared in any convenient manner, enters the bottom of the cylinder and comes out again at the top. It then passes through a filter to retain any dust it may carry away, and thence into a series of iron tubes built into a furnace, where they are heated to about  $200^{\circ}\text{C}$ . In these tubes the nickel carbonyl carried off by the carbonic oxide is completely decomposed, and the nickel deposited against the sides of the tubes is from time to time withdrawn, and is thus obtained in the pieces of tubing and the plates which you see on the table.

The carbonic oxide regenerated in these tubes is passed through another filter, thence through a lime purifier, to absorb any carbonic acid which may have been formed through the action of the finely divided nickel upon the carbonic oxide, and is then returned through a small fan into the bottom of the cylinder. The whole of this

plant is automatically kept in motion by means of an electric motor, the gearing of which you see here.

By means of this apparatus we have succeeded in extracting the nickel from a great variety of ores, in a time varying, according to the nature of the ore, between a few hours and several days.

Before the end of this year this process is going to be established in Birmingham on a scale that will enable me to place its industrial capacity beyond doubt, so that I feel justified in the expectation that in a few months nickel carbonyl, a substance quite unknown two years ago, and to-day still a great rarity, which has not yet passed out of the chemical laboratory, will be produced in very large quantities, and will play an important rôle in metallurgy.

The process possesses, besides its great simplicity, the additional advantage that it is possible to immediately obtain the nickel in any definite form. If we deposit it in tubes we obtain nickel tubes; if we deposit it in a globe we obtain a globe of nickel; if we deposit it in any heated mould we obtain copies of these moulds in pure, firmly coherent, metallic nickel. A deposit of nickel reproduces the most minute details of the surface of the moulds to fully the same extent as galvanic reproductions, so that all the very numerous objects now produced by galvanic deposition, of which Mr. Swan exhibited here such a large and beautiful variety a fortnight ago, can be produced by this process with the same perfection in pure metallic nickel. It is equally easy to nickel-plate any surface which will withstand the temperature of  $180^{\circ}$  C. by heating it to that temperature and exposing it to the vapour, or even to a solution of nickel carbonyl, a process which may in many cases have advantages over electro-plating. I have on the table before me specimens of nickel ores we have thus treated, of nickel tubes and plates we have obtained from these ores, and a few specimens of articles of pure nickel and articles plated with nickel which have been prepared in my laboratory. These will give you some idea of the prospects which the process I have described opens out to the metallurgist, upon whom, from day to day, greater demands are made to supply pure nickel in quantities. The most valuable properties of the alloy of nickel and iron, called nickel-steel, which promises to supply us with impenetrable iron-clads, have made an abundant and cheap supply of this metal a question of national importance. The inspection of the few specimens of articles of pure nickel, and of nickel-plated articles, will, I hope, suffice to show you the great facilities the process offers for producing very fine copies, and for making articles of such forms as cannot be produced by hydraulic pressure, the only method hitherto available for manufacturing articles of pure nickel.

I have also here a small coiled pipe made by my process, and kindly lent by Prof. Ramsay, which is interesting as being the first article made in this way for use in a chemical laboratory.

I began my lecture by bringing under your notice an idea of Liebig's, which he published fifty-eight years ago. I have shown



you how he himself elaborated this idea, and how it developed, until within recent years it has led to results of the highest scientific importance, and probably of great practical utility.

Had Liebig all these results before his "mind's eye" when he penned those prophetic words I have quoted? This is a question it is impossible to answer. Who will attempt to measure the range of vision of our great men, who from their lofty pinnacle see with eagle eye far into the Land of Science, and reveal to us wonderful sights which we can only realise after toiling slowly along the road they have indicated? Whether Liebig saw all these results or not, it is due to him, and to men like him, that science continues its marvellous advance, dispersing the darkness around us, and ever adding to the scope and exactness of our knowledge, that mighty power for promoting the progress and enhancing the happiness of humanity.

[L. M.]

Friday, June 10, 1892.

THE RIGHT HON. LORD KELVIN, D.C.L. LL.D. Pres. R.S.  
Vice-President, in the Chair.

PROFESSOR DEWAR, M.A. LL.D. F.R.S. M.R.I.

*Magnetic Properties of Liquid Oxygen.*

(Abstract.)

AFTER alluding to the generous aid which he had received both from the Royal Institution and from others in connection with his researches on the properties of liquid oxygen, and to the untiring assistance rendered him by his co-workers in the laboratory, Prof. Dewar said that on the occasion of the commemoration last year of the centenary of the birth of Michael Faraday he had demonstrated some of the properties of liquid oxygen. He hoped that evening to go several steps further, and to show liquid air, and to render visible some of its more extraordinary properties.

The apparatus employed consisted of the gas-engine downstairs, which was driving two compressors. The chamber containing the oxygen to be liquefied was surrounded by two circuits, one traversed by ethylene, the other by nitrous oxide. Some liquid ethylene was admitted to the chamber belonging to its circuit, and there evaporated. It was then returned to the compressor as gas and liquefied, and thence, again, into the chamber as required. A similar cycle of operations was carried out with the nitrous oxide. There was a hundredweight of liquid ethylene prepared for the experiment. Ethylene was obtained from alcohol by the action of strong sulphuric acid. Its manufacture was exceedingly difficult, because dangerous, and as the efficiency of the process only amounted to 15 or 20 per cent. the preparation of a hundredweight of liquid was no light task. The cycle of operations, which, for want of time, was not fully explained, was the same as that commonly employed in refrigerating machinery working with ether or ammonia.

The lecturer then exhibited to the audience a pint of liquid oxygen, which by its cloudy appearance showed that it contained traces of impurity. The oxygen was filtered, and then appeared as a clear transparent liquid with a slightly blue tinge. The density of oxygen gas at  $-182^{\circ}$  C. is normal, and the latent heat of volatilisation of the liquid is about 80 units. The capillarity of liquid oxygen at its boiling-point was about one-sixth that of water. The temperature of liquid oxygen at atmospheric pressure, determined by the specific heat method, using platinum and silver, was  $-180^{\circ}$  C.

Reference was then made to a remarkable experimental corroboration of the correctness for exceedingly low temperatures of Lord

Kelvin and Prof. Tait's thermo-electric diagram. If the lines of copper and platinum were prolonged in the direction of negative temperature, they would intersect at  $-95^{\circ}\text{C}$ . Similarly, the copper and palladium lines would cut one another at  $-170^{\circ}\text{C}$ . Now, if this diagram were correct, the E.M.F. of the thermo-electric junctions of these two pairs of metals should reverse at these points. A Cu-Pt junction connected to a reflecting galvanometer was then placed in oxygen vapour and cooled down. At  $-100^{\circ}\text{C}$ . the spot of light stopped and reversed. A Cu-Pd junction was afterwards placed in a tube containing liquid oxygen, and a similar reversal took place at about  $-170^{\circ}\text{C}$ .

Liquid oxygen is a non-conductor of electricity: a spark, taken from an induction coil, one millimetre long in the liquid requires a potential equal to a striking distance in air of 25 millimetres. It gave a flash now and then, when a bubble of the oxygen vapour in the boiling liquid came between the terminals. Thus liquid oxygen is a high insulator. When the spark is taken from a Wimshurst machine the oxygen appears to allow the passage of a discharge to take place with much greater ease. The spectrum of the spark taken in the liquid is a continuous one, showing all the absorption bands.

As to its absorption spectrum, the lines A and B of the solar spectrum are due to oxygen, and they came out strongly when the liquid was interposed in the path of the rays from the electric lamp. Both the liquid and the highly compressed gas show a series of five absorption bands, situated respectively in the orange, yellow, green and blue of the spectrum.

Experiments prove that gaseous and liquid oxygen have substantially the same absorption spectra. This is a very noteworthy conclusion considering that no compound of oxygen, so far as is known, gives the absorptions of oxygen. The persistency of the absorption through the stages of gaseous condensation towards complete liquidity implies a persistency of molecular constitution which we should hardly have expected. The absorptions of the class to which A and B belong must be those most easily assumed by the diatomic molecules ( $\text{O}_2$ ) of ordinary oxygen; whereas the diffuse bands above referred to, seeing they have intensities proportional to the square of the density of the gas, must depend on a change produced by compression. This may be brought about in two ways, either by the formation of more complex molecules, or by the constraint to which the molecules are subjected during their encounters with one another.

When the evaporation of liquid oxygen is accelerated by the action of a high expansion pump and an open test-tube is inserted into it, the tube begins to fill up with liquid atmospheric air, produced at the ordinary barometric pressure.

Dr. Jansson had recently been making prolonged and careful experiments on Mont Blanc, and he found that these oxygen lines disappeared more and more from the solar spectrum as he reached higher

altitudes. The lines at all elevations come out more strongly when the sun is low, because the rays then have to traverse greater thicknesses of the earth's atmosphere.

Michael Faraday's experiments made in 1849 on the action of magnetism on gases opened up a new field of investigation. The following table, in which + means "magnetic" and - means "negative," summarises the results of Faraday's experiments.

MAGNETIC RELATIONS OF GASES (FARADAY).

	In Air.	In Carbonic Acid	In Hydrogen	In Coal Gas.
Air .. .. .	0	+	+ weak	+
Nitrogen .. .. .	-	-	- strong	-
Oxygen .. .. .	+	+	+ strong	+ strong
Carbonic acid .. .. .	-	0	-	- weak
Carbonic oxide .. .. .	-	-	-	- weak
Nitric oxide .. .. .	- weak	+	+	..
Ethylene .. .. .	-	-	-	- weak
Ammonia .. .. .	-	-	-	..
Hydrochloric acid .. .. .	-	-	- weak	..

Becquerel was before Faraday in experimenting upon this subject. Becquerel allowed charcoal to absorb gases, and then examined the properties of such charcoal in the magnetic field. He thus discovered the magnetic properties of oxygen to be strong, even in relation to a solution of ferrous chloride, as set forth in the following table:—

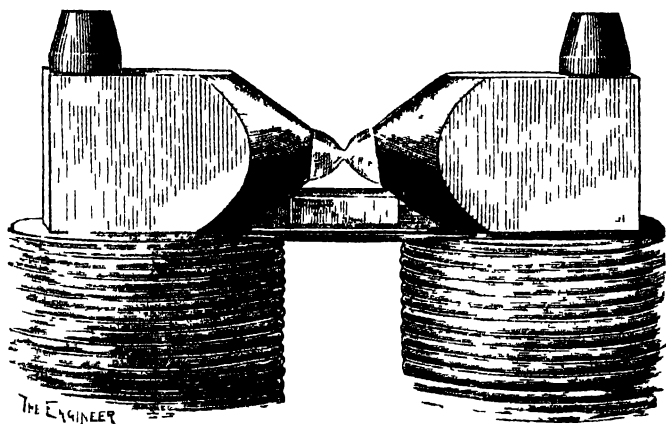
SPECIFIC MAGNETISM, EQUAL WEIGHTS (BECQUEREL).

Iron .. .. .	+ 1,000,000
Oxygen .. .. .	377
Ferrous chloride solution, sp. gr. 1.4334 .. .. .	+ 140
Air .. .. .	+ 88
Water .. .. .	- 3

The lecturer took a cup made of rock salt, and put in it some liquid oxygen. The liquid did not wet rock salt, but remained in a spheroidal state. The cup and its contents were placed between and a little below the poles of an electro-magnet. Whenever the circuit was completed, the liquid oxygen rose from the cup and connected the two poles, as represented in the cut, which is copied from a photograph of the phenomenon. Then it boiled away, sometimes more on one pole than the other, and when the circuit was broken it fell off the pole in drops back into the cup. He also showed that the magnet would draw up liquid oxygen out of a tube. A test-tube containing liquid oxygen when placed in the Hughes balance produced no disturbing effect. The magnetic moment of liquid oxygen is about 1000 when the magnetic moment of iron is taken as 1,000,000. On cooling some bodies increased in magnetic

power. Cotton wool, moistened with liquid oxygen, was strongly attracted by the magnet, and the liquid oxygen was actually sucked out of it on to the poles. A crystal of ferrous sulphate, similarly cooled, stuck to one of the poles.

The lecturer remarked that fluorine is so much like oxygen in its properties, that he ventured to predict that it will turn out to be a magnetic gas.



Magnetic Attraction of Liquid Oxygen.

Nitrogen liquefies at a lower temperature than oxygen, and one would expect the oxygen to come down before the nitrogen when air is liquefied, as stated in some text-books, but unfortunately it is not true. They liquefy together. In evaporating, however, the nitrogen boils off before the oxygen. He poured two or three ounces of liquid air into a large test-tube, and a smouldering splinter of wood dipped into the mouth of the tube was not re-ignited; the bulk of the nitrogen was nearly five minutes in boiling off, after which a smouldering splinter dipped into the mouth of the test-tube burst into flame.

Between the poles of the magnet all the liquefied air went to the poles; there was no separation of the oxygen and nitrogen. Liquid air has the same high insulating power as liquid oxygen. The phenomena presented by liquefied gases present an unlimited field for investigation. At  $-200^{\circ}\text{C}$ . the molecules of oxygen had only one-half of their ordinary velocity and had lost three-fourths of their energy. At such low temperatures they seemed to be drawing near what might be called "the death of matter," so far as chemical action was concerned; liquid oxygen, for instance, had no action upon a piece of phosphorus and potassium or sodium dropped into it; and once he thought and publicly stated, that at such temperatures all chemical action ceased. That statement

required some qualification because a photographic plate placed in liquid oxygen, could be acted upon by radiant energy, and at a temperature of  $-200^{\circ}\text{C.}$  was still sensitive to light.

Prof. M'Kendrick had tried the effect of these low temperatures upon the spores of microbic organisms, by submitting in sealed glass tubes blood, milk, flesh, and such-like substances, for one hour to a temperature of  $-182^{\circ}\text{C.}$ , and subsequently keeping them at blood heat for some days. The tubes on being opened were all putrid. Seeds also withstood the action of a similar amount of cold. He thought, therefore, that this experiment had proved the possibility of Lord Kelvin's suggestion, that life might have been brought to the newly-cooled earth upon a seed-bearing meteorite.

In concluding, the lecturer heartily thanked his two assistants, Mr. R. N. Lennox and Mr. J. W. Heath, for the arduous work they had had in preparing such elaborate demonstrations.

[J. D.]

Friday, January 20, 1893.

SIR FREDERICK BRAMWELL, Bart. D.C.L. LL.D. F.R.S. Honorary  
Secretary and Vice-President, in the Chair.

PROFESSOR DEWAR, M.A. LL.D. F.R.S. *M.R.I.*

*Liquid Atmospheric Air.*

THE prosecution of research at temperatures approaching the zero of absolute temperature is attended with difficulties and dangers of no ordinary kind. Having no recorded experience to guide us in conducting such investigations, the best instruments and methods of working have to be discovered. The necessity of devising some new kind of vessel for storing and manipulating exceedingly volatile fluids like liquid oxygen and liquid air, became apparent when the optical properties of the bodies came under examination. The liquids, being in active ebullition, were in a condition which rendered optical measurements impossible. All attempts at improvement on the principle of using a succession of surrounding glass vessels, the annular space between such vessels having the cool current of the vapour coming from the boiling liquid led through them, proved a failure. Apart altogether from the rapid ebullition interfering with experimental work, the fact that it took place involved a great additional cost in the conduct of experiments on the properties of matter under such exceptional conditions of temperature.

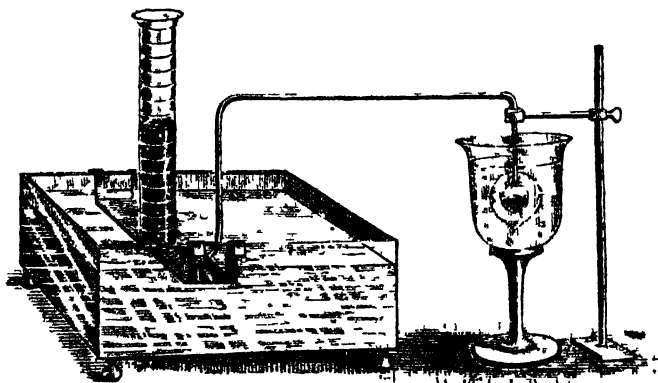
While suffering great anxiety on the question of expenditure, the Goldsmiths' Company came forward with the handsome contribution of 1000*l.* to continue the work with improved apparatus. Personally, I desire to express my grateful thanks to the Goldsmiths' Company for tendering such encouragement and support.

On careful consideration it became apparent that the proper way of attacking the problem was to conduct a series of experiments on the relative amounts of heat conveyed to boiling liquid gases; firstly, by means of the convective transference of heat by the gas particles, and, secondly, by radiation from surrounding bodies. The early experiments of Dulong and Petit on the laws of radiation had proved the very important part played by the gas particles surrounding a body in dissipating heat otherwise than by pure radiation. In the year 1873 I used a highly-exhausted vessel in calorimetric experiments "On the Physical Constants of Hydrogenium" (Trans.

Roy. Soc. Ed., vol. xxvii.) and the subsequent investigations of Crookes, and especially of Bottomley, having confirmed the great importance to be attached to the gas particles in the gain or loss of heat, it naturally occurred to me that the use of high vacua surrounding the vessels containing liquid gases would be advantageous. In order to arrive at definite data, some means of conducting comparative experiments between the amount of convective and radiant heat at such low temperatures had to be devised.

The apparatus shown in Fig. 1 measures the relative volumes of gas distilled in a given time under definite conditions, so that the measure of the gas distilled is proportional to the amount of heat conveyed to the liquid. The experiments are made in the following way. the distilling vessel to the right consists of two concentric spherical chambers, the space between being highly exhausted by a mercurial air-pump. The inner sphere is filled with liquid ethylene, oxygen,

FIG. 1.



or air, and the whole apparatus immersed in water maintained at a constant temperature. Distillation begins immediately and continues at a constant rate provided the liquid in the bulb is maintained at the same level. This is not possible, but a sufficient uniformity is attained by making the observations during the evaporation of the first fourth of the contents. Having measured the volume of gas produced per minute when the sphere is surrounded with a high vacuum, the vessel is taken out of the water, the end nipped off, air allowed to enter, and then again closed. In this condition the inner sphere is filled up again with the liquid and the above experiment repeated. The following results were obtained, using ethylene and oxygen respectively:

Liquid Oxygen, Vacuum .. ..	170	co. per minute.
" " Air .. ..	840	" "
" Ethylene, Vacuum .. ..	56	" "
" " Air .. ..	250	" "

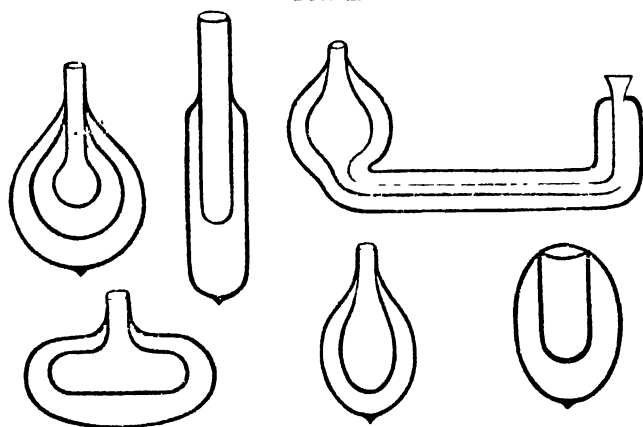
These results prove that under the same conditions a high vacuum



diminishes the rate of evaporation to one-fifth part of what it is when the substance is surrounded with air at atmospheric pressure, or, in other words, liquid oxygen or ethylene lasts five times longer when surrounded with a vacuous space.

The next step was to construct a series of glass vessels surrounded by a vacuous space, suitable for various experiments, and such are represented in Fig. 2. In vessels of this kind, if the vacuum is very high, no ice appears on the surface of the outer vessel, even although the walls of the vacuous space are within half an inch of each other, and the liquid oxygen or air evaporates almost solely from the surface, no bubbles of gas being given off throughout the mass of the liquid. So far the convective transference of heat has been stopped by the use of a high vacuum, but if the inner vessel is coated with a bright deposit of silver, then the radiation is

FIG. 2.



diminished also, with the result that the rate of evaporation is further reduced to more than a half. In such vessels liquid oxygen or liquid air can be kept for hours, and the economy and ease of manipulation greatly improved.

The arrangements represented in Fig. 2 may be employed to study the law of radiation at low temperatures. All that is necessary for the purpose is to immerse the outer vessel in liquids maintained at different temperatures. The following preliminary results have been obtained, using oxygen, which boils at  $-180^{\circ}\text{C}$ ., in the inner sphere:—

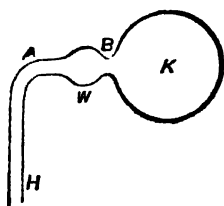
Temperature.						Radiation.
$-115^{\circ}\text{C}$ .	..	..	..	..	..	60 cc.
$-78^{\circ}\text{C}$ .	..	..	..	..	..	120 "
$+6^{\circ}\text{C}$ .	..	..	..	..	..	300 "
$+65^{\circ}\text{C}$ .	..	..	..	..	..	600 "

These results show that radiation (along with such convective transference as remains) grows approximately at the rate of the cube

of the absolute temperature. Many further experiments must, however, be made before the real law of radiation at low temperatures can be strictly defined.

To produce exceedingly high vacua in vessels used for such purposes as the collection and storage of liquid air, a mercurial vacuum made in the following manner has been found highly satisfactory. Take, as an illustration, a glass vessel shaped like Fig. 3, and after placing in it a quantity of mercury and connecting the pipe H with a good working air-pump, place in an oil or air-bath heated above  $200^{\circ}\text{C}$ ., and distil off a good quantity of the mercury. While the distillation is taking place the tube is sealed off at the point A, and the bulb instantly removed from the heated bath to such an extent as to allow condensation to take place in the small chamber marked B W in the figure. As the air-pump can maintain a vacuum of ten millimetres, no difficulty arises in sealing the glass tube during the continuance of the distillation. In some cases the mercury in the vessel has been heated up to near its boiling point in air, and then the air-pump started, causing thereby an almost explosive burst of mercurial vapour which very effectually carries all the air out of the vessel. After cooling, the vessel is removed from the bath, and the excess of mercury brought into the small bulb B W, care having been taken to remove any small globules of mercury, which adhere with great tenacity to the surface of the glass, by heating K while the part B W is kept cool. In this way the vessel K is filled with nothing but mercurial vapour, the pressure of which depends solely

FIG. 3.



on the temperature of the liquid mercury contained in the small enlargement, and as this can, if necessary, be cooled to  $-190^{\circ}\text{C}$ . by immersing it in liquid air, we have the means of creating in K a vacuum of inconceivable tenuity. It is sufficient for the production of very good vacua to cool the mercury in B W to  $-80^{\circ}\text{C}$ . (using solid carbonic acid as the cooling agent), and while in this condition to seal off the bulb containing all the condensed mercury, so that K is left full of saturated vapour at  $-80^{\circ}\text{C}$ . When very high exhaustions are required it is better not to seal off the mercury bulb, as the glass is apt to give off some kind of vapour. A similar mode of proceeding is adopted when other shaped vessels have to be highly exhausted, and no difficulty has arisen in operating with vessels having the capacity of more than a litre.

The perfection of the vacuum, assuming that nothing remains but molecules of mercury in the form of vapour, depends upon the temperature to which the subsidiary bulb is cooled. The well-known law which expresses approximately the relation between temperature and pressure in the case of saturated vapours, must be assumed to be applicable to mercury vapour at temperatures where direct measurement becomes impossible. Having calculated the constants of a

vapour pressure formula from observed data at high temperatures, it is easy to arrive at a value of the vapour pressure for any assumed lower temperature. Such a formula as the following,

$$\log. P = 15.1151 - 2.2931, \log. T - \frac{3665.7}{T},$$

where  $P$  is pressure in millimetres of mercury and  $T$  is the absolute temperature, agrees well with the experimental results. This formula gives the vapour pressure at  $0^{\circ}\text{C.}$  as  $0.000\ 18$  mill., and at  $-80^{\circ}\text{C.}$  as  $0.000\,000\,003$  mill., or respectively, about the sixth and the four hundred thousandth of a millionth of an atmosphere. Such a high vacuum could never be reached by the use of any form of mercurial air-pump. The electric discharge in such vacua produces intense phosphorescence of the glass, giving thereby a continuous spectrum, which makes the detection of the mercury lines difficult.

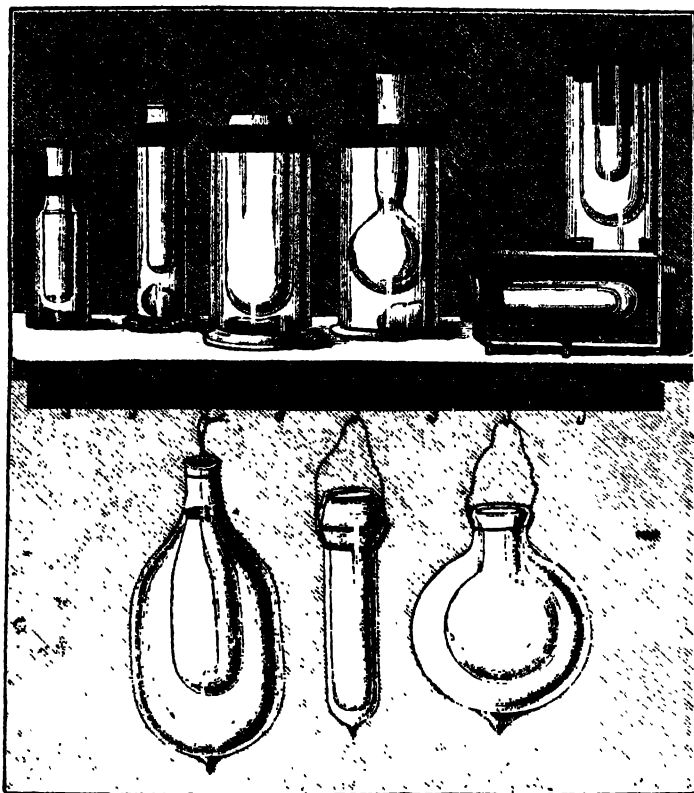
Consider for a moment the proofs that could be adduced that mercury vapour, even below a millionth of an atmosphere pressure, can behave like an ordinary saturated vapour. The most characteristic property of a space filled with any saturated vapour is that cooling to a lower temperature causes partial condensation of the vapour in the form of liquid or solid. The amount of vapour in a mercurial vacuum at the ordinary temperature would weigh about the tenth of a milligram in the volume of a litre, and to see such an amount of the metal it would require to be concentrated on a small area in the form of a fine metallic film. Experiment has shown that the fifth of a milligram of gold may be made to cover one square centimetre of surface, so that a minute quantity of metal can be observed if properly deposited. This can be easily achieved in such mercurial vacua by cooling a small portion of the surface of the glass to the temperature of  $-180^{\circ}\text{C.}$  by the application of a pad of cotton wool saturated with liquid oxygen. In an instant the vapour of mercury deposits in the form of a brilliant mirror, which, on the temperature rising, becomes subdivided into a mass of exceedingly minute spheres of liquid. A repetition of the cooling does not bring down a new mirror, provided the first area is maintained cool, but if the vessel contains excess of liquid mercury any number of mirrors of mercury may be deposited in succession.

Mercury is thus proved to distil at the ordinary temperature when the vapour pressure is under the millionth of an atmosphere. Further, it is easy to prove in this way that the cooling of the liquid mercury in such a subsidiary vessel (which has been described as a temporary part of the vacuum vessel) greatly improves the vacuum. For this purpose it is sufficient to cool the said vessel with some solid carbonic acid, and then to try and reproduce a mirror of mercury in the way previously described. No mercury mirror can be formed so long as the cold bath is maintained. If a piece of blotting paper, cut into any desired shape, be moistened with water, and then applied to the surface of one of the vacuum vessels

containing excess of liquid mercury, the local reduction of temperature produced by the evaporation soon causes a rough image of the paper to appear in the form of minute globules of condensed mercury. Such experiments support the view that the laws of saturated vapours are maintained at very low pressures.

In Fig. 4 specimens of the old and new vessels for collecting and manipulating liquid gases are shown. In each of the old forms it

FIG. 4.



will be noted that a mass of phosphoric anhydride placed in the lower portion is required to absorb traces of water, otherwise the vessels are useless for optical observations. The vacuum receivers get over this difficulty.

The perfection of the vacuum in different vessels, all treated in the same way, differs very much, and after use they almost invariably deteriorate. The relative rates of evaporation of liquid oxygen under the same conditions in different vessels is the best test of the vacuum. In many of the large vessels used for the storage of liquid gases, it

is convenient and more effective to cause the deposition of a mercury mirror over the surface of the inner vessel (by leaving a little liquid mercury in the lower part of the double-shaped flask), instead of silvering as previously described. Under such conditions the mercury instantly distils and forms a brilliant mirror all over the surface of the inner vessel. The fact that mercury has a very high refractive index and is a bad conductor of heat are factors of importance in retarding the conveyance of heat. After the mercury mirror has been formed any further increase in the thickness of the film can be prevented, and at the same time the vacuum improved by freezing the excess of liquid mercury in the lower part of the vessel. The vacuum vessels described equally retard the loss as well as the gain of heat, and are admirably adapted for all kinds of calorimetric observations. The future use of these vessels in thermal observations will add greatly to the accuracy and ease of conducting investigations. The double spherical form of vacuum vessel is excellent for showing that the elevation or depression of a given volume of air a few feet causes an increase or diminution of volume, due to the small change of atmospheric pressure. The volume of air in the inner sphere is guarded from any sudden change of temperature by the surrounding highly vacuous space. This is only one of the many uses to which such receivers can be put.

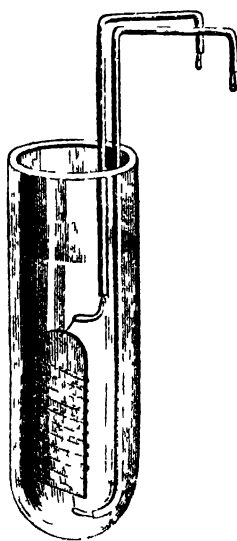
In making vacua, many other substances have been examined along with mercury, but they have not given equally satisfactory results.

Sulphur would occur to any one as a substance that might replace mercury, seeing the density in the form of vapour, and also the latent heat of vaporisation, are nearly identical; and it has the further advantage of being a solid at ordinary temperatures. The sulphur vacua have, however, so far not been an improvement, chiefly because traces of organic matter are decomposed by the sulphur, giving sulphuretted hydrogen and sulphurous acid, gases which are dissolved by and remain in the sulphur.

When the surface of such a sulphur vacuum is cooled with liquid oxygen in the manner previously described, a faint crystalline deposit occurs, only it takes a much longer time to appear than in the case of the mercury vacuum. If a similar vessel is boiled out, using phosphorus as the volatile substance, the application of liquid oxygen to the surface causes instant deposition. Thus it can be proved sulphur and phosphorus distil at ordinary temperatures just like mercury.

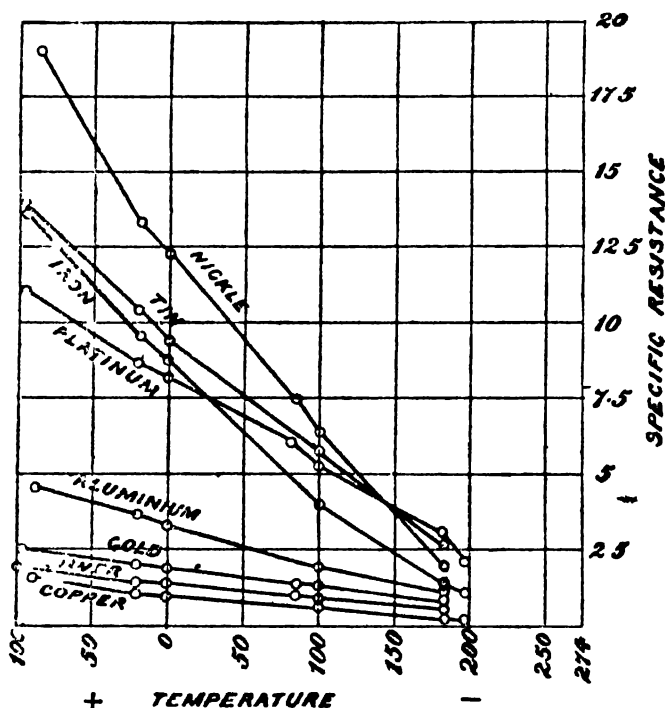
An investigation as to the electric conductivity of metals, alloys, and carbon at low temperatures has been undertaken in

FIG 5.



conjunction with my friend, Professor J. A. Fleming, D.Sc., F.R.S. The experiments are made by means of a resistance coil shown in Fig. 5, consisting of a piece of notched mica coiled with the fine wire to be tested, and of stout insulated copper-rod connections. The coil and connection are immersed in liquid oxygen contained in a vacuum test-tube, and the temperature of  $-200^{\circ}$  C. can be reached by exhausting the oxygen by means of a powerful air-pump. The results point to the conclusion that absolutely pure

FIG. 6.



Electrical Resistance of Metals at Low Temperature.

metals seem to have no resistance near the zero of temperature as indicated by the above curves (Fig. 6) obtained by experiment. With alloys there is little change in resistance, as indicated in the curves (Fig. 7). The conductivity of carbon decreases with low temperatures, and increases with high ones. At the temperature of the electric arc, carbon appears to have no resistance.

The optical constants of liquid oxygen, ethylene, and nitrous oxide have been so far determined, and in this matter my colleague, Professor Liveing, has been associated with me in the conduct of this work. The results obtained are given in the following table,

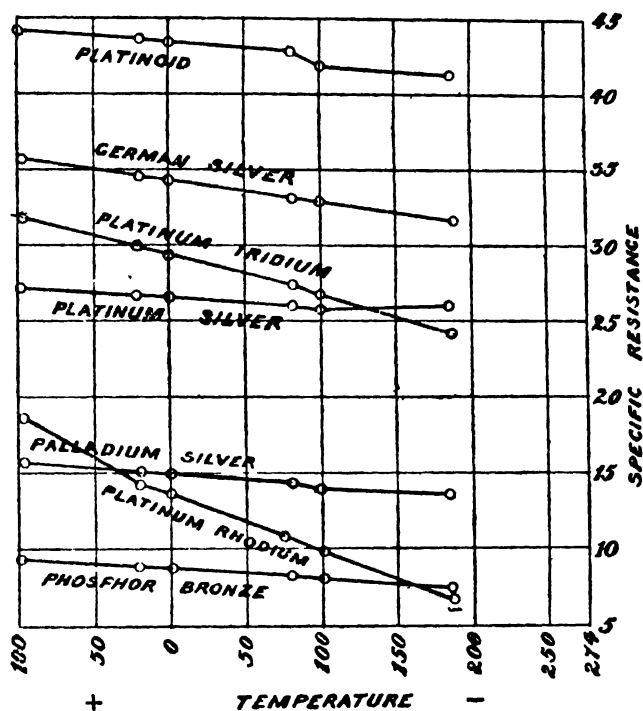
and tend to confirm the Law of Gladstone as being applicable to such substances :—

## REFRACTIVE INDICES OF LIQUID GASES.

	Index.	Ref. Constant.	Ref. Molecular.
Oxygen .. ..	1.2236 .. ..	1.989 .. ..	6.364
Ethylene .. ..	1.3632 .. ..	0.626 .. ..	17.528
Nitrous Oxide ..	1.3305 .. ..	0.263 .. ..	11.587

$$\text{Law of Gladstone } \frac{\mu - 1}{D} = \text{Constant.}$$

FIG. 7.



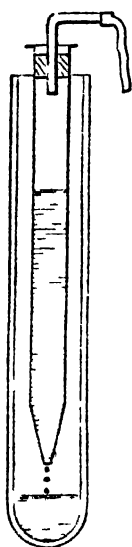
Electrical Resistance of Alloys at Low Temperatures.

The determination of the refractive index of liquid oxygen, at its boiling-point of  $-182^{\circ}\text{C}$ ., presented more difficulty than would have been anticipated. The necessity for enclosing the vessel containing the liquid in an outer case to prevent the deposit of a layer of hoar-frost which would scatter all the rays falling on it, rendered manipulation difficult; and hollow prisms with cemented sides cracked with the extreme cold. It was only after repeated attempts, involving the expenditure of a whole litre of liquid oxygen on each experiment, that we succeeded in getting an approximate measure of the refractive index for the D line of sodium.

The mean of several observations gave the minimum deviation with a prism of  $59^{\circ} 15'$  to be  $15^{\circ} 11' 30''$ , and thence  $\mu = 1.2236$ . The density of liquid oxygen at its boiling-point of  $-182^{\circ} \text{C}$ . is 1.124, and this gives for the refraction-constant,  $\frac{\mu^2 - 1}{d} = 1.989$ , and for the refraction-equivalent 3.182. This corresponds closely with the refraction-equivalent deduced by Landolt from the refractive indices of a number of organic compounds. Also it differs little from the refraction-equivalent for gaseous oxygen, which is 3.0316. This is quite consistent with the supposition that the molecules of oxygen in the liquid state are the same as in the gaseous.

If we take the formula  $\frac{\mu^2 - 1}{(\mu^2 + 2)d}$  for the refraction-constant we

FIG. 8.



find the value of it for liquid oxygen to be .1265, and the corresponding refraction-equivalent 2.024. These are exactly the means of the values found by Mascart and Lorenz for gaseous oxygen. The inherent difficulties of manipulation, and the fact that the sides of the hollow prism invariably became coated with a solid deposit, which obscured the image of the source of light, have hitherto prevented our determining the refractive indices for rays other than D.

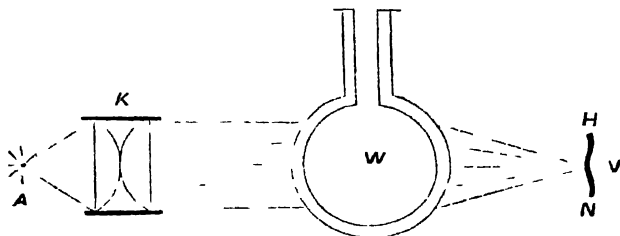
The optical projection of vacuum vessels having the shape of a double test-tube are very suitable for lecture illustration. As the critical point of oxygen is some thirty degrees higher than nitrogen it is easier to liquefy, and, consequently, becomes the most convenient substance to use for the production of temperatures about  $-200^{\circ} \text{C}$ . Liquid nitrogen, carbonic oxide, or air can conveniently be made at the ordinary atmospheric pressure, provided they are brought into a vessel cooled by liquid oxygen boiling under the pressure of about half an inch of mercury.

A simple arrangement for this purpose is shown in Fig. 8. The inner tube contains the liquid oxygen under exhaustion, surrounded by a vacuum vessel, the interior space between the inner tube and the vacuum vessel being connected with a receiver containing the gas which is to be liquefied. If the object is to collect liquid air, the inner air space is left quite open, no precautions being needed to free the air from carbonic acid or moisture, because under the conditions such substances are solids, and only cause a slight opalescence in the liquid, which drops continuously from the end of the inner tube and accumulates in the vacuum vessel. If the air supply is forced to bubble through a little strong sulphuric acid, the rate of condensation and the relative volume of gas and liquid can be observed. Liquid air boils at the temperature of  $-190^{\circ} \text{C}$ ., giving off substantially pure nitrogen.



As the nitrogen boils  $10^{\circ}$  C. lower than oxygen, after a time the liquid alters its composition and boiling point, finally becoming pure oxygen. During the evaporation the liquid air changes very remarkably in colour, passing from a very faint blue to a much deeper shade. The changes can be traced best by the marked increase in the width of the absorption bands of liquid oxygen. If air, collected in the above manner in a vacuum vessel, is isolated from a rapid heat supply by immersing the vessel in liquid oxygen, and then a powerful air-pump brought to act upon it, after a time it passes into the condition of a clear, transparent, solid ice. Nitrogen solidifies, under such conditions, into a white mass of crystals, but all attempts to solidify oxygen by its own evaporation have failed. Such liquids as air and oxygen, we should anticipate, would be especially transparent to heat radiation, seeing they are very diathermic substances in their gaseous state. The thermal transparency of liquid oxygen can be shown by passing the radiation from the electric arc, as shown in the diagram, through a spherical

FIG. 9.



vacuum vessel filled with clear filtered liquid, thereby concentrating the rays at a focus and igniting a piece of black paper held there.

In this experiment the oxygen lens has a temperature of  $-18^{\circ}$  C., yet it does not prevent the concentrated radiation reaching a red heat at the focus. At such low temperatures as boiling oxygen and air all chemical action ceases. If some liquid oxygen is cooled to  $-200^{\circ}$  C., and a glowing piece of wood inserted into the vessel above the liquid, it refuses to burst into flame, because of the low pressure of the vapour. An interesting experiment may be made by immersing an electric pile, composed of carbon and sodium, into liquid oxygen, when almost immediately the electric current ceases. The gaseous oxygen coming from the liquid must be exceedingly pure and dry, and as it has been alleged two chemical substances require the presence of a third one in order that they may combine, it was interesting to ascertain if a substance like sulphur would continue to burn after ignition in such an atmosphere. Sulphur placed in a small platinum vessel that had just been heated to redness, was raised to the boiling point, and in the act of combustion lowered into a vacuum vessel containing liquid oxygen. The com-

bustion continued active, and for a time could be maintained in the middle of the liquid oxygen. This result suggests that oxygen and sulphur can enter into combination in a perfectly dry condition. Some notion of the temperature of liquid air is given by running on to the surface some absolute alcohol, which, after rolling about in the spheroidal state, suddenly solidifies into a hard transparent ice, which rattles on the sides of the vacuum test-tube like a marble. On lifting the solid alcohol out by means of a looped wire the application of the flame of a Bunsen burner will not ignite it. After a time the solid melts and falls from the looped wire like a thick syrup.

It is not the question of the change of state in matter, however interesting, that in our day has special attractions for the chemist, but the means of studying the properties of matter generally under the conditions of such exceptionally low temperatures as are the concomitants of the transition in the case of substances like oxygen and nitrogen. The work of investigation in this field proceeds slowly but surely, and one need not despair (unless on the grounds of expense) in the future of adding further data to our knowledge of the properties of matter near the zero of absolute temperature.

At the commencement of the lecture reference was made to the dangers and difficulties of this kind of research, and it becomes a pleasant duty to acknowledge the great services rendered by my assistants. But for the persistency and determination of Mr. Lennox, coupled with his marked engineering ability, the work would not have made such progress, and he has been ably supported by Mr. Heath.

[J. D.]

Friday, February 17, 1893.

WILLIAM HUGGINS, Esq. D.C.L. LL.D. F.R.S. Vice-President,  
in the Chair.

PROFESSOR A. H. CHURCH, M.A. F.R.S. M.R.I.

*Turacin, a remarkable Animal Pigment containing Copper.*

THE study of natural colouring matters is at once peculiarly fascinating and peculiarly difficult. The nature of the colouring matters in animals and plants, and even in some minerals (ruby, sapphire, emerald and amethyst, for example) is still, in the majority of cases, not completely fathomed.

Animal pigments are generally less easily extracted and are more complex than those of plants. They appear invariably to contain nitrogen—an observation in accord with the comparative richness in that element of animal cells and their contents. Then, too, much of the coloration of animals, being due to microscopic structure, and therefore having a mechanical and not a pigmentary origin, differs essentially from the coloration of plants. Those animal colours which are primarily due to structure do, however, involve the presence of a dark pigment—brown or black—which acts at once as a foil and as an absorbent of those incident rays which are not reflected.

Many spectroscopic examinations of animal pigments have been made. Except in the case of blood- and bile-pigments, very few have been submitted to exhaustive chemical study. Spectral analysis, when uncontrolled by chemical, and when the influence of the solvent employed is not taken into account, is very likely to mislead the investigator. And, unfortunately, the non-crystalline character of many animal pigments, and the difficulty of purifying them by means of the formation of salts and of separations by the use of appropriate solvents, oppose serious obstacles to elucidation. Of blood-red or hæmoglobin it cannot be said that we know the centesimal composition, much less the molecular weight. Even of hæmatin the empirical formula has not yet been firmly established. The group of black and brown pigments to which the various melanins belong still awaits adequate investigation. We know they contain nitrogen ( $8\frac{1}{2}$  to 13 per cent.), and sometimes iron, but the analytical results do not warrant the suggestion of empirical formulæ for them. The more nearly they appear to approach purity the freer the majority of them seem from any fixed constituent such as iron or other metal. It is to be regretted that Dr. Krukenberg, to whom we are indebted for much valuable work

on several pigments extracted from feathers, has not submitted the interesting substances he has described to quantitative chemical analysis.

I must not, however, dwell further upon these preliminary matters. I have introduced them mainly in order to indicate how little precise information has yet been gathered as to the constitution of the greater number of animal pigments, and how difficult is their study.

And now let me draw your attention to a pigment which I had the good fortune to discover, and to the investigation of which I have devoted I am afraid to say how many years.

It was so long ago as the year 1866 that the solubility in water of the red colouring matter in the wing-feathers of a *plantain-eater* was pointed out to me. [One of these feathers, freed from grease, was shown to yield its pigment to pure water.] I soon found that alkaline liquids were more effective solvents than pure water, and that the pigment could be precipitated from its solution by the addition of an acid. [The pigment was extracted from a feather by very dilute ammonia, and then precipitated by adding excess of hydrochloric acid.] The next step was to filter off the separated colouring matter, and to wash and dry it. The processes of washing and drying are tedious and cannot be shown in a lecture. But the product obtained was a solid of a dark crimson hue, non-crystalline, and having a purple semi-metallic lustre. I named it *turacin* (in a paper published in a now long-defunct periodical 'The Student and Intellectual Observer,' of April, 1868). The name was taken from "*Touraco*," the appellation by which the *plantain-eaters* are known—the most extensive genus of this family of birds being *Turacus*.

From the striking resemblance between the colour of arterial blood and that of the red touraco feathers, I was led to compare their spectra. Two similar absorption bands were present in both cases, but their positions and intensities differed somewhat. Naturally I sought for iron in my new pigment. I burnt a portion, dissolved the ash in hydrochloric acid, and then added sodium acetate and potassium ferrocyanide. To my astonishment I got a precipitate, not of Prussian blue, but of Prussian brown. This indication of the presence of copper in turacin was confirmed by many tests, the metal itself being also obtained by electrolysis. It was obvious that the proportion of copper present in the pigment was very considerable—greatly in excess of that of the iron (less than .5 per cent.) in the pigment of blood.

Thus far two striking peculiarities of the pigment had been revealed, namely, its easy removal from the web of the feather, and the presence in it of a notable quantity of copper. Both facts remain unique in the history of animal pigments. The solubility was readily admitted on all hands, not so the presence of copper. It was suggested that it was derived from the Bunsen burner used in the incineration, or from some preservative solution applied to the bird-skins. And it was asked "How did the copper get into the feathers?" The doubters might have satisfied themselves as to

copper being normally and invariably present by applying a few easy tests and by the expenditure of half-a-crown in acquiring a touraco wing. My results were, however, confirmed (in 1872) by several independent observers, including Mr. W. Crookes, Dr. Gladstone, and Mr. Greville Williams. And in 1873 Mr. Henry Bassett, at the request of the late Mr. J. J. Monteiro, pushed the inquiry somewhat further. I quote from Monteiro's 'Angola and the River Congo,' published in 1875 (vol. ii. pp. 75-77). "I purchased a large bunch of the red wing-feathers in the market at Sierra Leone, with which Mr. H. Bassett has verified Professor Church's results conclusively," &c., &c. Mr. Bassett's results were published in the *Chemical News* in 1873, three years after the appearance of my research in the *Phil. Trans.* As concentrated hydrochloric acid removes no copper from turacin, even on boiling, the metal present could not have been a mere casual impurity; as the proportion is constant in the turacin obtained from different species of touraco, the existence of a single definite compound is indicated. The presence of traces of copper in a very large number of plants as well as of animals has been incontestably established. And, as I pointed out in 1868, copper can be readily detected in the ash of banana fruits, the favourite food of several species of the "turacin-bearers." The feathers of a single bird contain on the average two grains of turacin, corresponding to  $\cdot 14$  of a grain of metallic copper; or, putting the amount of pigment present at its highest, just one-fifth of a grain. This is not a large amount to be furnished by its food to one of these birds once annually during the season of renewal of its feathers. I am bound, however, to say that in the blood and tissues of one of these birds, which I analysed immediately after death, I could not detect more than faint traces of copper. The particular specimen examined was in full plumage; I conclude that the copper in its food, not being then wanted, was not assimilated.

Let us now look a little more closely at these curious birds themselves. Their nearest allies are the cuckoos, with which they were formerly united by systematists. It has, however, been long conceded that they constitute a family of equal rank with the Cuculidæ. According to the classification adopted in the Natural History Museum, the order Picariæ contains eight sub-orders, the last of which, the Cocyges, consists of two families, the Cuculidæ and the Musophagidæ. To the same order belong the Hoopoes, the Trogons, the Wood-peckers. The plantain-eaters or Musophagidæ are arranged in six genera and comprise 25 species. In three genera—*Turacus*, *Gallirex*, and *Musophaga*—comprising eighteen species, and following one another in zoological sequence, turacin occurs; from three genera (seven species)—*Corythæola*, *Schizorhis*, and *Gymno-schizorhis*—the pigment is absent. [The coloured illustrations to H. Schlegel's *Monograph* (Amsterdam, 1860) on the Musophagidæ were exhibited]. The family is confined to Africa: 8 of the turacin-bearers are found in the west sub-region, 1 in the south-west, 2 in

the south, 2 in the south-east, 4 in the east, 2 in the central, and 2 in the north-east. It is noteworthy that, in all these sub-regions save the south-east, turacin-bearers are found along with those plantain-eaters which do not contain the pigment. Oddly enough two of the latter species, *Schizorhis africana* and *S. zonura*, possess white patches destitute of pigment in those parts of the feathers which in the turacin-bearers are crimson. These birds do not, I will not say cannot, decorate these bare patches with this curiously complex pigment. [Some extracts were here given from the late Mr. Monteiro's book on Angola, vol. ii. pp. 74-79, and from letters by Dr. B. Hinde. These extracts contained references to curious traits of the touracos.]

Usually from 12 to 18 of the primaries or metacarpo-digitals and secondaries or cubitals amongst the wing feathers of the turacin-bearers have the crimson patches in their web. Occasionally the crimson patches are limited to six or seven of the eleven primaries. I have observed this particularly with the violet plantain-eater (*Musophaga violacea*). In these cases the crimson head-feathers, which also owe their colour to turacin, are few in number, as if the bird, otherwise healthy, had been unable to manufacture a sufficiency of the pigment. I may here add that the red tips of the crest feathers of *Turacus meriani* also contain turacin.

In all the birds in which turacin occurs, this pigment is strictly confined to the red parts of the web, and is there unaccompanied by any other colouring matter. It is therefore found that if a single barb from a feather be analysed its black base and its black termination possess no copper, while the intermediate portion gives the blue-green flash of copper when incinerated in the Bunsen flame. [A parti-coloured feather was burnt in the Bunsen flame, with the result indicated]

Where it occurs, turacin is homogeneously distributed in the barbs, barbicels and crochets of the web, and is not found in granules or corpuscles.

To the natural question "Does turacin occur in any other birds besides the touracos?" a negative answer must at present be given. At least my search for this pigment in scores of birds more or less nearly related to the Musophagidæ has met with no success. In some of the plantain-eaters (species of *Turacus* and *Gallirex*) there is, however, a second pigment closely related to turacin. It is of a dull grass-green colour, and was named Turacoverdin by Dr. Krukenberg in 1881. I had obtained this pigment in 1868 by boiling turacin with a solution of caustic soda, and had figured its characteristic absorption band in my first paper (Phil. Trans., vol. clix. 1870, p. 630, fig. 4). My product was, however, mixed with unaltered turacin. But Dr. Krukenberg obtained what certainly seems to be the same pigment from the green feathers of *Turacus corythaix*, by treating them with a 2 per cent. solution of caustic soda. I find, however, that a solution of this strength

dissolves, even in the cold, not only a brown pigment associated with turacoverdin, but ultimately the whole substance of the web. By using a much weaker solution of alkali (1 part to a thousand of water) a far better result is obtained. [The characteristic absorption band of turacoverdin, which lies on the less refrangible side of D, was shown; also the absorption bands of various preparations of turacin.] I have refrained from the further investigation of turacoverdin, hoping that Dr. Krukenberg would complete his study of it. At present I can only express my opinion that it is identical with the green pigment into which turacin when moist is converted by long exposure to the air or by ebullition with soda, and which seems to be present in traces in all preparations of *isolated* turacin however carefully prepared.

A few observations may now be introduced on the physical and chemical characters of turacin. It is a colloid of colloids. And it enjoys in a high degree one of the peculiar properties of colloids, that of retaining, when freshly precipitated, an immense proportion of water. Consequently, when its solution in ammonia is precipitated by an acid, the coagulum formed is very voluminous. [The experiment was shown.] One gram of turacin is capable of forming a semi-solid mass with 600 grams of water. Another character which turacin shares with many other colloids is its solubility in pure water and its insolubility in the presence of mere traces of saline matter. It would be tedious to enumerate all the observed properties of turacin, but its deportment on being heated and the action of sulphuric acid upon it demand particular attention.

At 100° C., and at considerably higher temperatures, turacin suffers no change. When, however, it is heated to the boiling-point of mercury it is wholly altered. No vapours are evolved, but the substance becomes black and is no longer soluble in alkaline liquids, nor, when still more strongly heated afterwards, can it be made to yield the purple vapours which unchanged turacin gives off under the same circumstances. This peculiarity of turacin caused great difficulty in its analysis, for these purple vapours contain an organic crystalline compound in which both nitrogen and copper are present, and which resist further decomposition by heat. [Turacin was so heated as to show its purple vapours, and also the green flame with which they burn.] This production of a volatile organic compound of copper is perhaps comparable with the formation of nickel- and ferro-carbonyl.

The action of concentrated sulphuric acid upon turacin presents some remarkable features. The pigment dissolves with a fine crimson colour, and yields a new compound, the spectrum of which presents a very close resemblance to that of hæmatoporphyrin [Turacin was dissolved in oil of vitriol: the spectrum of an ammoniacal solution of the turacoporphyrin thus produced was also shown], the product obtained by the same treatment from hæmatin: in other respects also this new derivative of turacin, which I call turacoporphyrin, reminds one

of hæmatoporphyrin. But, unlike this derivative of hæmatin, it seems to retain some of its metallic constituent. The analogy between the two bodies cannot be very close, for if they were so nearly related as might be argued from the spectral observations, hæmatin ought to contain not more but less metal than is found to be present therein.

The percentage composition of turacin is probably—Carbon 53·69, hydrogen 4·6, copper 7·01, nitrogen 6·96, and oxygen 27·74. These numbers correspond pretty nearly to the empirical formula,  $C_{32} H_{31} Cu_2 N_9 O_{32}$ . But I lay no stress upon this expression.

I have before said that copper is very widely distributed in the Animal Kingdom. Dr. Giunti, of Naples, largely extended (1881) our knowledge on this point. I can hardly doubt that this metal will be found in traces in all animals. But besides turacin only one organic copper compound has been as yet recognised in animals. This is a respiratory, and not a mere decorative, pigment like turacin. Léon Fredericq discovered this substance, called hæmocyanin. It has been observed in several genera of Crustacea, Arachnida, Gastropoda and Cephalopoda. I do not think it has ever been obtained in a state of purity, and I cannot accept for it the fantastic formula— $C_{367} H_{1369} Cu_8 S_4 O_{258}$ —which has recently been assigned to it. On the other hand, I do not sympathise with the doubts as to its nature which F. Heim has recently formulated in the *Comptes Rendus*.

It is noteworthy, in connection with the periodic law, that all the essential elements of animal and vegetable organic compounds have rather low atomic weights, iron, manganese and copper representing the superior limit. Perhaps natural organic compounds containing manganese will some day be isolated, but at present such bodies are limited to a few containing iron, and to two, hæmocyanin and turacin, of which copper forms an essential part.

If I have not yet unravelled the whole mystery of the occurrence and properties of this strange pigment, it must be remembered that it is very rare and costly, and withal difficult to prepare in a state of assured purity. It belongs, moreover, to a class of bodies which my late master, Dr. A. W. von Hofmann, quaintly designated as “dirts” (a magnificent dirt truly!)—substances which refuse to crystallize and cannot be distilled. I have experienced likewise, during the course of this investigation, frequent reminders of another definition propounded by the same great chemist, when he described organic research as “a more or less circuitous route to the sink!”

I am very glad to have had the opportunity of sharing with an audience in this Institution the few glimpses I have caught from time to time during the progress of a tedious and still incomplete research into the nature of a pigment which presents physiological and chemical problems of high if not of unique interest.

Let my last word be a word of thanks. I am indebted to several friends for aid in this investigation, and particularly to Dr. MacMunn, of Wolverhampton, the recognised expert in the spectroscopy of animal pigments.

[A. H. C.]



Friday, March 24, 1893.

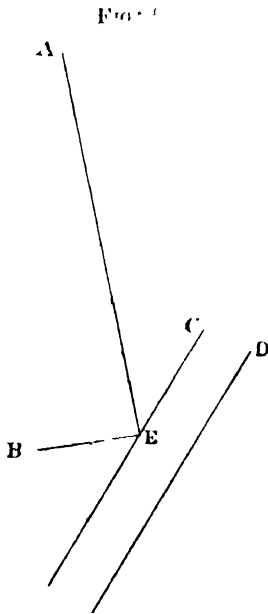
SIR FREDERICK BRAMWELL, Bart. D.C.L. LL.D. F.R.S. Honorary  
Secretary and Vice-President, in the Chair.

The Right Hon. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*

*Interference Bands and their Applications.*

(Abstract.)

THE formation of the interference bands, known as Newton's Rings, when two slightly curved glass plates are pressed into contact, was illustrated by an acoustical analogue. A high-pressure flame B (Fig. 1) is sensitive to sounds which reach it in the direction EB, but is insensitive to similar sounds which reach it in the nearly perpendicular direction AB.



A is a "bird call," giving a pure sound (inaudible) of wave-length ( $\lambda$ ) equal to about 1 cm.: C and D are reflectors of perforated zinc. If C acts alone the flame is visibly excited by the waves reflected from it, though by far the greater part of the energy is transmitted. If D, held parallel to C, be then brought into action, the result depends upon the interval between the two partial reflectors. The reflected sounds may co-operate, in which case the flame flares vigorously; or they may interfere, so that the flame recovers, and behaves as if no sound at all were falling upon it. The first effect occurs when the reflectors are close together, or are separated by any multiple of  $\frac{1}{2} \sqrt{2} \lambda$ ; the second when the interval is midway between those of the above-mentioned series, that is, when it coincides with an odd multiple of  $\frac{1}{4} \sqrt{2} \lambda$ . The factor  $\sqrt{2}$  depends upon the obliquity of the reflection.

The coloured rings, as usually formed between glass plates, lose a good deal of their richness by contamination with white light reflected from the exterior surfaces.

The reflection from the hindermost surface is easily got rid of by employing an opaque glass, but the reflection from the first surface is less easy to deal with. One plan, used in the lecture, depends upon the use of slightly wedge-shaped glasses ( $2^\circ$ )

so combined that the exterior surfaces are parallel to one another, but inclined to the interior operative surfaces. In this arrangement the false light is thrown somewhat to one side, and can be stopped by a screen suitably held at the place where the image of the electric arc is formed.

The formation of colour and the ultimate disappearance of the bands as the interval between the surfaces increases, depends upon the mixed character of white light. For each colour the bands are upon a scale proportional to the wave-length for that colour. If we wish to observe the bands when the interval is considerable—bands of high interference as they are called—the most natural course is to employ approximately homogeneous light, such as that afforded by a soda flame. Unfortunately, this light is hardly bright enough for projection upon a large scale.

A partial escape from this difficulty is afforded by Newton's observations as to what occurs when a ring system is regarded through a prism. In this case the bands upon one side may become approximately achromatic, and are thus visible to a tolerably high order, in spite of the whiteness of the light. Under these circumstances there is, of course, no difficulty in obtaining sufficient illumination; and bands formed in this way were projected upon the screen.\*

The bands seen when light from a soda flame falls upon nearly parallel surfaces have often been employed as a test of flatness. Two flat surfaces can be made to fit, and then the bands are few and broad, if not entirely absent; and, however the surfaces may be presented to one another, the bands should be straight, parallel, and equidistant. If this condition be violated, one or other of the surfaces deviates from flatness. In Fig. 2, A and B represents the glasses to be tested, and C is a lens of 2 or 3 feet focal length. Rays diverging from a soda flame at E are rendered parallel by the lens, and after reflection from the surfaces are recombined by the lens at E. To make an observation, the coincidence of the radiant point and its image must be somewhat disturbed, the one being displaced to a position a little beyond, and the other to a position a little in front of, the diagram.

The eye, protected from the flame by a suitable screen, is placed at the image, and being focused upon A B, sees the field traversed by bands. The reflector D is introduced as a matter of convenience to make the line of vision horizontal.

These bands may be photographed. The lens of the camera takes the place of the eye, and should be as close to the flame as possible. With suitable plates, sensitised by cyanin, the exposure required may vary from ten minutes to an hour. To get the best results, the hinder surface of A should be blackened, and the front surface of B should be thrown out of action by the superposition of a wedge-shaped

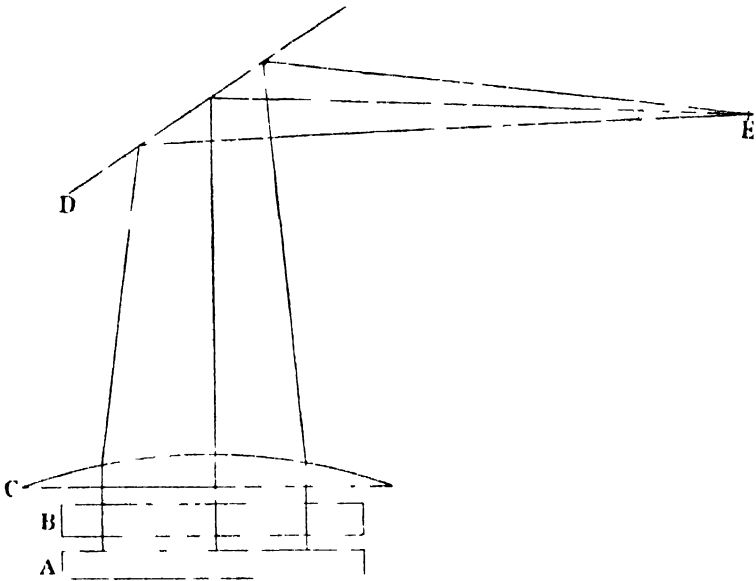
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\* The theory is given in a paper upon "Achromatic Interference Bands," *Phil. Mag.* Aug. 1889.

plate of glass, the intervening space being filled with oil of turpentine or other fluid having nearly the same refraction as glass. Moreover, the light should be purified from blue rays by a trough containing solution of bichromate of potash. With these precautions the dark parts of the bands are very black, and the exposure may be prolonged much beyond what would otherwise be admissible.

The lantern slides exhibited showed the elliptical rings indicative of a curvature of the same sign in both directions, the hyperbolic bands corresponding to a saddle-shaped surface, and the approximately parallel system due to the juxtaposition of two telescopic "flats," kindly lent by Mr. Common. On other plates were seen grooves due

FIG. 2.



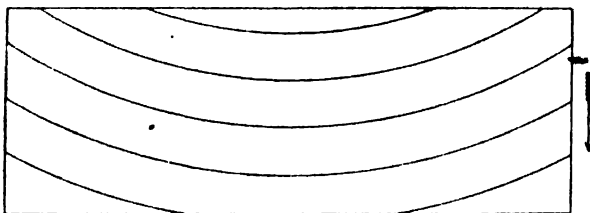
to rubbing with rouge along a defined track, and depressions, some of considerable regularity, obtained by the action of diluted hydrofluoric acid, which was allowed to stand for some minutes as a drop upon the surface of the glass.

By this method it is easy to compare one flat with another, and thus, if the first be known to be free from error, to determine the errors of the second. But how are we to obtain and verify a standard? The plan usually followed is to bring *three* surfaces into comparison. The fact that two surfaces can be made to fit another in all azimuths proves that they are spherical and of equal curvatures, but one convex and the other concave, the case of perfect flatness not being excluded. If A and B fit another, and also A and C, it follows that B and C must be similar. Hence, if B and C also fit one

another, all three surfaces must be flat. By an extension of this process the errors of three surfaces which are not flat can be found from a consideration of the interference bands which they present when combined in three pairs.

But although the method just referred to is theoretically complete, its application in practice is extremely tedious, especially when the surfaces are not of revolution. A very simple solution of the difficulty has been found in the use of a free surface of water, which, when protected from tremors and notes, is as flat as can be desired.\* In order to avoid all trace of capillary curvature it is desirable to allow a margin of about  $1\frac{1}{2}$  inch. The surface to be tested is supported horizontally at a short distance ( $\frac{1}{10}$  or  $\frac{1}{20}$  inch) below that of the water, and the whole is carried upon a large and massive levelling stand. By the aid of screws the glass surface is brought into approximate parallelism with the water. In practice the principal trouble is in the avoidance of tremors and notes. When the apparatus is set up on the floor of a cellar in the country, the tremors are sufficiently excluded, but care must be taken to protect the surface from the slightest draught. To this end the space over the water must be enclosed almost air-tight. In towns, during the hours of traffic, it would probably require great precaution to avoid the disturbing effects of tremors. In this respect it is advantageous to diminish the thickness of the layer of water; but if the thinning be carried too far, the subsidence of the water surface to equilibrium becomes sur-

FIG. 3.



prisingly slow, and a doubt may be felt whether after all there may not remain some deviation from flatness due to irregularities of temperature.

With the aid of the levelling screws the bands may be made as broad as the nature of the surface admits; but it is usually better so to adjust the level that the field is traversed by five or six approximately parallel bands. Fig. 3 represents bands actually observed from the face of a prism. That these are not straight, parallel, and equidistant is a proof that the surface deviates from flatness. The question next

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\* The diameter would need to be 4 feet in order that the depression at the circumference, due to the general curvature of the earth, should amount to  $\frac{1}{20}$   $\lambda$ .

arising is to determine the direction of the deviation. This may be effected by observing the displacement of the bands due to a known motion of the levelling screws; but a simpler process is open to us. It is evident that if the surface under test were to be moved downwards parallel to itself, so as to increase the thickness of the layer of water, every band would move in a certain direction, viz. *towards* the side where the layer is thinnest. What amounts to the same, the retardation may be increased, without touching the apparatus, by so moving the eye as to *diminish* the obliquity of the reflection. Suppose, for example, in Fig. 3, that the movement in question causes the bands to travel downwards, as indicated by the arrow. The inference is that the surface is concave. More glass must be removed at the ends of the bands than in the middle in order to straighten them. If the object be to correct the errors by local polishing operations upon the surface, the rule is that *the bands, or any parts of them, may be rubbed in the direction of the arrow.*

A good many surfaces have thus been operated upon; and although a fair amount of success has been attained, further experiment is required in order to determine the best procedure. There is a tendency to leave the marginal parts behind; so that the bands though straight over the greater part of their length, remain curved at their extremities. In some cases hydrofluoric acid has been, resorted to, but it appears to be rather difficult to control.

The delicacy of the test is sufficient for every optical purpose. A deviation from straightness amounting to  $\frac{1}{10}$  of a band interval could hardly escape the eye, even on simple inspection. This corresponds to a departure from flatness of  $\frac{1}{20}$  of a wave-length in water, or about  $\frac{1}{30}$  of the wave-length in air. Probably a deviation of  $\frac{1}{100} \lambda$  could be made apparent.

For practical purposes a layer of moderate thickness, adjusted so that the two systems of bands corresponding to the duplicity of the soda line do not interfere, is the most suitable. But if we wish to observe bands of high interference, not only must the thickness be increased, but certain precautions become necessary. For instance, the influence of obliquity must be considered. If this element were absolutely constant, it would entail no ill effect. But in consequence of the finite diameter of the pupil of the eye, various obliquities are mixed up together, even if attention be confined to one part of the field. When the thickness of the layer is increased, it becomes necessary to reduce the obliquity to a minimum, and further to diminish the aperture of the eye by the interposition of a suitable slit. The effect of obliquity is shown by the formula

$$2t(1 - \cos \theta) = n\lambda.$$

The necessary parallelism of the operative surfaces may be obtained, as in the above described apparatus, by the aid of levelling. But a much simpler device may be employed, by which the experimental

difficulties are greatly reduced. If we superpose a layer of water upon a surface of mercury, the flatness and parallelism of the surfaces take care of themselves. The objection that the two surfaces would reflect very unequally may be obviated by the addition of so much dissolved colouring matter, e.g. soluble aniline blue, to the water as shall equalise the intensities of the two reflected lights. If the adjustments are properly made, the whole field, with the exception of a margin near the sides of the containing vessel, may be brought to one degree of brightness, being in fact all included within a fraction of a band. The width of the margin, within which rings appear, is about one inch, in agreement with calculation founded upon the known values of the capillary constants. During the establishment of equilibrium after a disturbance, bands are seen due to variable thickness, and when the layer is thin, persist for a considerable time.

When the thickness of the layer is increased beyond a certain point, the difficulty above discussed, depending upon obliquity, becomes excessive, and it is advisable to change the manner of observation to that adopted by Michelson. In this case the eye is focused, not, as before, upon the operative surfaces, but upon the flame, or rather upon its image at E (Fig. 2). For this purpose it is only necessary to introduce an eye-piece of low power, which with the lens C (in its second operation) may be regarded as a telescope. The bands now seen depend entirely upon obliquity according to the formula above written, and therefore take the form of circular arcs. Since the thickness of the layer is absolutely constant, there is nothing to interfere with the perfection of the bands except want of homogeneity in the light.

But, as Fizeau found many years ago, the latter difficulty soon becomes serious. At a very moderate thickness it becomes necessary to reduce the supply of soda, and even with a very feeble flame a limit is soon reached. When the thickness was pushed as far as possible, the retardation, calculated from the volume of liquid and the diameter of the vessel, was found to be 50,000 wave-lengths, almost exactly the limit fixed by Fizeau.

To carry the experiment further requires still more homogeneous sources of light. It is well known that Michelson has recently observed interference with retardations previously unheard of, and with the aid of an instrument of ingenious construction has obtained most interesting information with respect to the structure of various spectral lines.

A curious observation respecting the action of hydrofluoric acid upon polished glass surfaces was mentioned in conclusion. After the operation of the acid the surfaces appear to be covered with fine scratches, in a manner which at first suggested the idea that the glass had been left in a specially tender condition, and had become scratched during the subsequent wiping. But it soon appeared that the effect was a *development* of scratches previously existent in a latent state. Thus parallel lines ruled with a knife edge, at first invisible even in

a favourable light, became conspicuous after treatment with acid. Perhaps the simplest way of regarding the matter is to consider the case of a furrow with perpendicular sides and a flat bottom. If the acid may be supposed to eat in equally in all directions, the effect will be to *broaden* the furrow, while the depth remains unaltered. It is possible that this method might be employed with advantage to *intensify* (if a photographic term may be permitted) gratings ruled upon glass for the formation of spectra.

Friday, June 2, 1893.

SIR DOUGLAS GALTON, K.C.B. D.C.L. LL.D. F.R.S. Vice-President,  
in the Chair.

PROFESSOR OSBORNE REYNOLDS, M.A. LL.D. F.R.S.

*Study of Fluid Motion by means of Coloured Bands.*

IN his charming story of 'The Purloined Letter,' Edgar Allan Poe tells how all the efforts and artifices of the Paris police to obtain possession of a certain letter, known to be in a particular room, were completely baffled for months by the simple plan of leaving the letter in an unsealed envelope in a letter-rack, and so destroying all *curiosity* as to its contents; and how the letter was at last found there by a young man who was not a professional member of the force. Closely analogous to this is the story I have to set before you to-night—how certain mysteries of fluid motion, which have resisted all attempts to penetrate them are at last explained by the simplest means and in the most obvious manner.

This indeed is no new story in science. The method adopted by the minister, *D.*, to secrete his letter appears to be the favourite of Nature in keeping her secrets, and the history of science teems with instances in which keys, after being long sought amongst the grander phenomena, have been found at last not hidden with care, but scattered about, almost openly, in the most commonplace incidents of every-day life which have excited no curiosity.

This was the case in physical astronomy—to which I shall return after having reminded you that the motion of matter in the universe naturally divides itself into three classes.

1. The motion of bodies as a whole—as a grand illustration of which we have the heavenly bodies, or more humble, but not less effective, the motion of a pendulum, or a falling body.

2. The relative motion of the different parts of the same fluid or elastic body—for the illustration of which we may go to the grand phenomena presented by the tide, the whirlwind, or the transmission of sound, but which is equally well illustrated by the oscillatory motion of the wave, as shown by the motion of its surface, and by the motion of this jelly, which, although the most homely illustration, affords by far the best illustration of the properties of an elastic solid.

3. The inter-motions of a number of bodies amongst each other—to which class belong the motions of the molecules of matter



resulting from heat, as the motions of the molecules of a gas, in illustration of which I may mention the motions of individuals in a crowd, and illustrate by the motion of the grains in this bottle when it is shaken, during which the white grains at the top gradually mingle with the black ones at the bottom—which interdiffusion takes an important part in the method of coloured bands.

Now of these three classes of motion that of the individual body is incomparably the simplest. Yet, as presented in the phenomena of the heavens, which have ever excited the greatest curiosity of mankind, it defied the attempts of all philosophers for thousands of years, until Galileo discovered the laws of motion of mundane matter. It was not until he had done this and applied these laws to the heavenly bodies that their motions received a rational explanation. Then Newton, taking up Galileo's parable and completing it, found that its strict application to the heavenly bodies revealed the law of gravitation, and developed the theory of dynamics.

Next to the motions of the heavenly bodies, the wave, the whirlwinds, and the motions of clouds, had excited the philosophical curiosity of mankind from the earliest time. Both Galileo and Newton, as well as their followers, attempted to explain these by the laws of motion, but although the results so obtained have been of the utmost importance in the development of the theory of dynamics it was not till this century that any considerable advance was made in the application of this theory to the explanation of fluid phenomena, and although during the last fifty years splendid work has been done, work which, in respect of the mental effort involved, or the scientific importance of the results, goes beyond that which resulted in the discovery of Neptune, yet the circumstances of fluid motion are so obscure and complex that the theory has yet been interpreted only in the simplest cases.

To illustrate the difference between the interpretation of the theory of the heavenly bodies and that of fluid motion, I would call your attention to the fact that solid bodies, on the behaviour of which the theory of the motion of the planets is founded, move as one piece, so that their motion is exactly represented by the motion of their surfaces; that they are not affected with any internal disorder which may affect their general motion. So surely is this the case, that even those who have never heard of dynamics can predict with certainty how any ordinary body will behave under any ordinary circumstances, so much so that any departure is a matter of surprise. Thus I have here a cube of wood, to one side of which a string is attached. Now hold it on one side, and holding the string you naturally suppose that when I let go it will turn down so as to hang with the string vertical; it does not do so, that is a matter of surprise; I place it on the other side and it still remains as I place it. If I swing it as a pendulum it does not behave like one.

Would Galileo have discovered the laws of motion had his pendulum behaved like this? Why is its motion peculiar? There is

internal motion. Of what sort? Well, I think my illustration may carry more weight if I do not tell you; you can all, I have no doubt, form a good idea. It is not fluid motion or I should feel bound to explain it. You have here an ordinary looking object which behaves in an extraordinary manner, which is yet very decided and clear, to judge by the motion of its surface, and from the manner of the motion I wish you to judge of the cause of the observed motion.

This is the problem presented by fluids, in which there may be internal motion which has to be taken into account before the motion of the surface can be explained. You can see no more of what the motion is within a homogeneous fluid, however opaque or clear, than you can see what is going on within the box. Thus, without colour bands the only visual clue to what is going on within the fluids is the motion of their bounding surfaces. Nor is this all; in most cases the surfaces which bound the fluid are immovable.

In the case of the wave on water the motion of the surface shows that there is motion, but because the surface shows no wave it does not do to infer that the fluid is at rest.

The only surfaces of the air within this room are the surfaces of the floor, walls, and objects within it. By moving the objects we move the air, but how far the air is at rest you cannot tell unless it is something familiar to you.

Now I will ask you to look at these balloons. They are familiar objects enough, and yet they are most sensitive anemometers, more sensitive than anything else in the room; but even they do not show any motion; each of them forms an internal bounding surface of the air. I send an *aerial messenger* to them, and a small but energetic motion is seen by which it acknowledges the message, and the same message travels through the rest, as if a *ghost* touched them. It is a wave that moves them. You do not feel it, and, but for the surfaces of the air formed by the balloons, would have no notion of its existence.

In this tank of beautifully clear distilled water, I project a heavy ball in from the end, and it shows the existence of the water by stopping almost dead within two feet. The fact that it is stopped by the water, being familiar, does not raise the question, Why does it stop?—a question to which, even at the present day, a complete answer is not forthcoming. The question is, however, suggested, and forcibly suggested, when it appears that with no greater or other evidence of its existence, I can project a disturbance through the water which will drive this small disc the whole length of the tank.

I have now shown instances of fluid motion of which the manner is in no way evident without colour bands, and were revealed by colour bands, as I showed in this room sixteen years ago. At that time I was occupied in setting before you the manners of motion revealed, and I could only incidentally notice the means by which this revelation was accomplished.

Amongst the ordinary phenomena of motion there are many which render evident the internal motion of fluids. Small objects suspended in the fluid are important, and that their importance has long been recognised is shown by the proverb—straws show which way the wind blows. Bubbles in water, smoke and clouds, afford the most striking phenomena, and it is doubtless these that have furnished philosophers with such clues as they have had. But the indications furnished by these phenomena are imperfect, and, what is more important, they only occur casually, and in general only under circumstances of such extreme complexity that any deduction as to the elementary motions involved is impossible. They afford indication of commotion, and perhaps of the general direction in which the commotion is tending, but this is about all.

For example, the different types of clouds ; these have always been noticed and are all named. And it is certain that each type of clouds is an indication of a particular type of motion in the air ; but no deductions as to what definite manner of motion is indicated by each type of cloud have ever been published.

Before this can be done it is necessary to reverse the problem, and find to what particular type of cloud a particular manner of motion would give rise. Now a cloud, as we see it, does not directly indicate the internal motion of which it is the result. As we look at clouds, it is not in general their motion that we notice, but their figure. It is hard to see that this figure changes while we are watching a cloud, though such a change is continually going on, but is apparently very slow on account of the great distance of the cloud and its great size. However, types of clouds are determined by their figure, not by their motion. Now what their figure shows is not motion, but is the history or result of the motion of particular strata of the air in and through surrounding strata. Hence, to interpret the figures of the clouds we must study the changes in shape of fluid masses, surrounded by fluid, which result from particular motions.

The ideal in the method of colour bands is to render streaks or lines in definite position in the fluid visible, without in any way otherwise interfering with these properties as part of the homogeneous fluid. If we could by a wish create coloured lines in the water these would be ideal colour bands. We cannot do this, nor can we exactly paint lines in the air or water.

I take this ladle full of highly coloured water, lower it slowly into the surface of the surrounding water till that within is level with that without ; then turn the ladle carefully round the coloured water ; the mass of coloured water will remain where placed.

I distribute the colour slowly. It does not mix with the clear water, and although the lines are irregular they stand out very beautifully. Their edges are sharp here. But in this large sphere, which was coloured before the lecture, although the coloured lines have generally kept their places, they have, as it were, swollen out and become merged in the surrounding water in consequence of molecular

motion. The sphere shows, however, one of the rarest phenomena in Nature—the internal state in almost absolute internal rest. The forms resemble nothing so much as stratus clouds, as seen on a summer day, though the continuity of the colour bands is more marked. A mass of coloured water once introduced is never broken. The discontinuity of clouds is thus seen to be due to other causes than mere motion.

Now, having called your attention to the rarity of water at rest, I will call your attention to what is apt to be a very striking phenomenon, namely, that when water is contained, like this, in a spherical vessel of which you cannot alter the shape, it is impossible by moving the vessel suddenly to set up relative motion in the interior of the water. I may swing this vessel about and turn it, but the colour band in the middle remains as it was, and when I stop shows the water to be at rest.

This is not so if the water has a free surface, or if the fluid is of unequal density. Then a motion of the vessel sets up waves, and the colour band shows at once the beautifully lawful character of the internal motion. The colour bands move backwards and forwards, showing how the water is distorted like a jelly, and as the wave dies out the colour bands remain as they were to begin with.

This illustrates one of the two classes of internal motion of water or fluid. Wherever fluid is not in contact with surfaces over which it has to glide, or which surfaces fold on themselves, the internal motions are of this purely wave character. The colour bands, however much they may be distorted, cannot be relatively displaced, twisted, or curled up, and in this case motion in water once set up continues almost without resistance. That wave motion in water with a free surface, is one of the most difficult things to stop is directly connected with the difficulty of setting still water in motion; in either case the influence must come through the surfaces. Thus it is that waves once set up will traverse thousands of miles, establishing communication between the shores of Europe and America. Wave motion in water is subject to enormously less resistance than any other form of material motion.

In wave motion, if the colour bands are across the wave they show the motion of the water; nevertheless, their chief indication is of the change of shape while the fluid is in motion.

This is illustrated in this long bottle, with the coloured water less heavy than the clear water. If I lay it down in order to establish equilibrium, the blue water has to leave the upper end of the bottle and spread itself over the clear water, while the clear water runs under the coloured. This sets up wave motion, which continues after the bottle has come to rest. But as the colour bands are parallel with the direction of motion of the waves, the motion only becomes evident in thickening and bending of the colour bands.

The waves are entirely between the two fluids, there being no motion in the outer surfaces of the bottle, which is everywhere glass.

They are owing to the slight differences in the density of the fluids, as is indicated by the extreme slowness of the motion. Of such kind are the waves in the air, that cause the clouds which make the mackerel sky, the vapour in the tops of the waves being condensed and evaporated again as it descends, showing the results of the motion.

The distortional motions, such as alone occur in simple wave motion, or where the surfaces of the fluid do not fold in on themselves, or wind in, are the same as occur in any homogeneous continuous material which completely fills the space between the surfaces.

If plastic material is homogeneous in colour it shows nothing as to the internal motion; but if I take a lump built of plates, blue and white, say a square, then I can change the surfaces to any shape without folding or turning the lump, and the coloured bands which extend throughout the lump show the internal changes. Now the first point to illustrate is that, however I change its shape, if I bring it back to the original shape the colour bands will all come back to their original positions, and there is no limit to the extent of the change that may thus be effected. I may roll this out to any length, or draw it out, and the diminution in thickness of the colour bands shows the extent of the distortion. This is the first and simplest class of motion to which fluids are susceptible. By this motion alone the elements of the fluid may be, and are, drawn out to an indefinitely fine line, or spread out in an indefinitely thin sheet, but they will remain of the same general figure.

By reversing the process they change back again to the original form. No colour band can ever be broken, even if the outer surface be punched in till the punch head comes down on the table; still all the colour bands are continuous under the punch, and there is no folding or lapping of the colour bands unless the external surface is folded.

The general idea of mixture is so familiar to us that the vast generalisation to which these ideas afford the key, remains unnoticed. That continued mixing results in uniformity, and that uniformity is only to be obtained by mixing, will be generally acknowledged, but how deeply and universally this enters into all the arts can but rarely have been apprehended. Does it ever occur to any one that the beautiful uniformity of our textile fabrics has only been obtained by the development of processes of mixing the fibres. Or, again, the uniformity in our construction of metals; has it ever occurred to any one that the inventions of Arkwright and Cort were but the application of the long-known processes by which mixing is effected in culinary operations? Arkwright applied the draw-rollers to uniformly extend the length of the cotton sliver at the expense of the thickness; Cort applied the rolling-mill to extend the length of the iron bloom at the expense of its breadth; but who invented the rolling-pin by which the pastry-cook extends the length at the expense of the thickness of the dough for the pie-crust?

In all these processes the object, too, is the same throughout—to obtain some particular shape, but chiefly to obtain a uniform texture. To obtain this nicety of texture it is necessary to mix up the material, and to accomplish this it is necessary to attenuate the material, so that the different parts may be brought together.

The readiness with which fluids are mixed and uniformity obtained is a by-word; but it is only when we come to see the colour bands that we realise that the process by which this is attained is essentially the same as that so laboriously discovered for the arts—as depending first on the attenuation of each element of the fluid—as I have illustrated by distortion.

In fluids, no less than in cooking, spinning and rolling—this attenuation is only the first step in the process of mixing—all involve the second process, that of folding, piling, or wrapping, by which the attenuated layers are brought together. This does not occur in the pure wave motion of water, and constitutes the second of the two classes of motion. If a wave on water is driven beyond a certain height it leaps or breaks, folding in its surface. Or, if I but move a solid surface through the water it introduces tangential motion, which enables the fluid to wind its elements round an axis. In these ways, and only in these ways, we are released from the restriction of not turning or lapping. And in our illustration, we may fold up our dough, or lap it—roll it out again and lap it again; cut up our iron bar, pile it, and roll it out again, or bring as many as we please of the attenuated fibres of cotton together to be further drawn. It may be thought that this attenuation and wrapping will never make perfect admixture, for however thin each element will preserve its characteristic, the coloured layers will be there, however often I double and roll out the dough. This is true. — But in the case of some fluids, and only in the case of some fluids, the physical process of diffusion completes the admixture. These colour bands have remained in this water, swelling but still distinct; this shows the slowness of diffusion. Yet such is the facility with which the fluid will go through the process of attenuating its elements and enfolding them, that by simply stirring with a spoon these colour bands can be drawn and folded so true that the diffusion will be instantaneous, and the fluid become uniformly tinted. All internal fluid motion other than simple distortion, as in wave motion, is a process of mixing, and it is thus from the arts we get the clue to the elementary forms and processes of fluid motion.

When I put the spoon in and mixed the fluid you could not see what went on—it was too quick. To make this clear, it is necessary that the motion should be very slow. The motion should also be in planes, at right angles to the direction in which you are looking. Such is the instability of fluid that to accomplish this at first appeared to be difficult. At last, however, as the result of much thought, I found a simple process which I will now show you, in what I think is a novel experiment, and you will see, what I think

has never been seen before by any one but Mr. Foster and myself, namely, the complete process of the formation of a cylindrical vortex sheet resulting from the motion of a solid surface. To make it visible to all I am obliged to limit the colour band to one section of the sheet, otherwise only those immediately in front would be able to see between the convolutions of the spiral. But you will understand that what is seen is a section, a similar state of motion extending right across the tank. From the surface you see the plane vane extending half-way down right across the tank ; this is attached to a float.

I now institute a colour band on the right of the vane out of the tube. There is no motion in the water, and the colour descends slowly from the tube. I now give a small impulse to the float to move it to the right, and at once the spiral form is seen from the tube. Similar spirals would be formed all across the tank if there were colours. The float has moved out of the way, leaving the revolving spiral with its centre stationary, showing the horizontal axis of the spiral is half-way between the bottom and surface of the tank, in which the water is now simply revolving round this axis.

This is the vortex in its simplest and rarest form (for a vortex cannot exist with its ends exposed). Like an army it must have its flanks protected ; hence a straight vortex can only exist where it has two surfaces to cover its flanks, and parallel vertical surfaces are not common in nature. The vortex can bend, and, as with a horse-shoe axis, can rest both its flanks on the same surface, as this piece of clay, or with a ring axis, which is its commonest form, as in the smoke ring. In both these cases the vortex will be in motion through the fluid, and less easy to observe.

These vortices have no motion beyond the rotation because they are half-way down the tank. If the vane were shorter they would follow the vane ; if it were longer they would leave it.

In the same way, if instead of one vortex there were two vortices, with their axes parallel, extending right across, the one above another, they would move together along the tank.

I replace the float by another which has a vane suspended from it, so that the water can pass both above and below the vane extending right across the middle portion of the tank. In this case I institute two colour bands, one to pass over the top, the other underneath, the vane, which colour bands will render visible a section of each vortex just as in the last case. I now set the float in motion and the two vortices turn towards each other in opposite directions. They are formed by the water moving over the surface of the vane, downwards to get under it, upwards to get over it, so that the rotation in the upper vortex is opposite to that in the lower. All this is just the same as before, but that instead of these vortices standing still as before they follow at a definite distance from the vane, which continues its motion along the tank without resistance.

Now this experiment shows, in the simplest form, the *modus*

*operandi* by which internal waves can exist in fluid without any motion in the external boundary. Not only is this plate moving flatwise through the water, but it is followed by all the water, coloured and uncoloured, enclosed in these cylindrical vortices. Now, although there is no absolute surface visible, yet there is a definite surface which encloses these moving vortices, and separates them from the water which moves out of their way. This surface will be rendered visible in another experiment I shall show you. Thus the water which has only wave motion is bounded by a definite surface, the motion of which corresponds to the wave; but inside this closed surface there is also water, so that we cannot see the surface, and this water inside is moving round and round, but so that its motion at the bounding surface is everywhere the same as that of the outside water.

The two masses of water do not mix. That outside moves out of the way of and past the vortices over the bounding surface, while the vortices move round and round inside the surface in such a way that it is moving in exactly the same manner at the surface as the wave surface outside.

This is the key to the internal motion of water. You cannot have a pure wave motion inside a mass of fluid with its boundaries at rest, but you have a compound motion, a wave motion outside, and a vortex within, which fulfils the condition that there shall be no sliding of the fluid over fluid at the boundary.

A means which I hope may make the essential conditions of this motion clearer occurred to me while preparing this lecture, and to this I will now ask your attention. I have here a number of layers of cotton-wool (wadding). Now I can force any body along between these layers of wadding. They yield, as by a wave, and let it go through; but the wadding must slide over the surface of the body so moving through it. And this it must *not* do if it illustrate the conditions of fluid motion. Now there is one way, and only one way, in which material can be got through between the sheets of wadding without slipping. It must roll through; but this is not enough, because if it rolls on the under surface it will be slipping on the upper. But if we have two rollers, one on the top of the other, between the sheets, then the lower roller rolls on the bottom sheet, the upper roller rolls against the upper sheet, so that there is no slipping between the rollers or the wadding, and, equally important, there is no slipping between the rollers, as they roll on each other. I have only to place a sheet of canvas between the rollers and draw it through; both the flannel rollers roll on the canvas and on the wadding, which they pass through without slipping, causing the wadding to move in a wave outside them, and affording a complete parable of the vortex motion.

I will now show by colour bands some of the more striking phenomena of internal motion, as presented by Nature's favourite form of vortex, the vortex ring, which may be described as two horseshoe vortices with their ends founded on each other.



To show the surface separating the water moving with the vortex from that which gives way outside, I discharge from this orifice a mass of coloured water, which has a vortex ring in it formed by the surface as already described. You see the beautifully defined mass moving on slowly through the fluid, with the proper vortex ring motion, but very slow. It will not go far before a change takes place, owing to the diffusion of the vortex motion across the bounding surface; then the coloured surface will be wound into the ring which will appear. The mass approaches the disc in front. It cannot pass, but will come up and carry the disc forward; but the disc, although it does not destroy the ring, disturbs the motion.

If I send a more energetic ring it will explain the phenomenon I showed you at the beginning of this lecture; it carries the disc forward as if struck with a hammer. This blow is not simply the weight of the coloured ring, but of the whole moving mass and the wave outside. The ring cannot pass the disc without destruction with the attendant wave.

Not only can a ring follow a disc, but as with the plane vane so with the disc, if we start a disc we must start a ring behind it.

I will now fulfil my promise to reveal the silent messenger I sent to those balloons. The messenger appears in the form of a large smoke ring, which is a vortex ring in air rendered visible by smoke instead of colour. The origination of these rings has been carefully set so that the balloons are beyond the surface which separates the moving mass of water from the wave, so that they are subject to the wave motion only. If they are within this surface they will disturb the direction of the ring, if they do not break it up.

These are, if I may say so, the phenomenal instances of internal motion of fluids. Phenomenal in their simplicity they are of intense interest, like the pendulum, as furnishing the clue to the more complex. It is by the light we gather from their study that we can hope to interpret the parable of the vortex wrapped up in the wave, as applied to the wind of heaven, and the grand phenomenon of the clouds, as well as those things which directly concern us, such as the resistance of our ships.

[O. R.]

Friday, January 19, 1894.

SIR FREDERICK BRAMWELL, BART. D.C.L. LL.D. F.R.S.

Honorary Secretary and Vice-President, in the Chair.

PROFESSOR DEWAR, M.A. LL.D. F.R.S. M.R.I.

*Scientific Uses of Liquid Air.*

WHEN Faraday was working on liquid gases in this Institution about 1823, with such means as were then at his command, his inquiry was limited to the determination of the specific gravities and vapour pressures of such bodies. Twenty years later, by the use of solid carbonic acid, the greatest cold then possible was obtained, and Faraday made admirable use of Thilorier's new cooling agent to extend his early investigations. Just as liquid carbonic acid produced in glass tubes was of no use as an agent for effecting the liquefaction of more resisting gaseous matters, until it could be manipulated in the solid state, so liquid air, until it could be handled, stored and used in open vessels, like any ordinary liquid, could not be said to possess scientific uses in any wide sense. Such operations become easy when double-walled vacuum vessels (such as were described in a former lecture) are employed in the conduct of experiments where substances boiling at very low temperatures have to be manipulated. The chief scientific use of liquid air consists in the facilities it gives for the study of the properties of matter at temperatures approaching the zero of absolute temperature. In this lecture the expression liquid air may mean either oxygen or air. Where a constant temperature is required oxygen is used. Liquid air made on the large scale may contain, after it is collected in open vacuum vessels, as much as 50 per cent. of oxygen. Such a liquid boils between  $-192^{\circ}$  and  $-182^{\circ}$  C., and the longer it is stored the nearer it comes to  $-182^{\circ}$  C. or the boiling point of pure oxygen. For a number of experiments of a qualitative character, whether it is liquid air or oxygen that is used makes no difference. In many of the experiments to be recorded, liquid oxygen made from the evaporation of liquid air was employed. In pursuing this subject in consort with Professor Fleming,\* a long series of experiments, involving the use of large supplies of liquid oxygen, have been carried out on the electric resistance of metals and alloys, and

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\* 'The Electrical Resistance of Metals and Alloys at Temperatures Approaching the Absolute Zero.' By James Dewar, LL.D. F.R.S. and J. A. Fleming, M.A. D.Sc. F.R.S. Professor of Electrical Engineering in University College, London. Phil. Mag. 1892.

the results warrant the conclusion that at the zero of absolute temperature all the pure metals would be perfect conductors of electricity. Under such conditions a current of electricity started in a pure metallic circuit would develop no heat, and therefore undergo no dissipation. Similarly, we infer there would be no Peltier effect at the zero. In other words, the passage of electricity from one metal to another would take place without evolution or absorption of heat.

Further investigation, along with Professor Liveing,\* on the refractive index of liquid nitrogen and air, has led to the conclusion that the refractive indices of nitrogen and air are respectively for the D-ray, 1.2053 and 1.2062. In these determinations, instead of using the prisms we have employed the method of Terguen and Trannin, which consists in suspending in the liquid two plates of glass with a thin layer of air between them, and measuring the angle of incidence at which the chosen ray suffers total reflection at the surface of the air. As all the vacuum vessels are either spherical or cylindrical in form when filled with liquid, they act as lenses which are irregular and full of striations. Further, small bubbles of gas being given off in the liquid rendered any image indistinct when viewed with a telescope. In order to avoid the necessity of observing any image through the liquid, it was used simply as a lens to concentrate the light observed on the slit of a spectroscope. Under such conditions the observations were easily executed and the results satisfactory.

For some time a series of observations on the thermal opacity of liquid oxygen and nitrogen have been projected. It is, however, exceedingly difficult to experiment in such a way as to eliminate the absorbing action of the glass vessels, and as the use of rock salt is impracticable, the absorption of heat of low refrangibility remains for the present undetermined. It is possible, however, to use the glass vacuum vessels to determine approximately the relative thermal transparency for heat of high refrangibility, such as is radiated by a colza lamp. The following results represent the heat transmitted through the same vacuum vessels filled with different liquids, taking chloroform as the unit for comparison and correcting for differences of refractive index.

Chloroform .. ..	1.0	Liquid nitrous oxide ..	0.93
Carbon bisulphide ..	1.6	Liquid ethylene .. ..	0.60
Liquid oxygen .. ..	0.9	Ether .. .. ..	0.50

From this result it follows that liquid oxygen is nearly as transparent to high temperature heat radiation as chloroform, which is one of the most transparent liquids next to carbon bisulphide. Liquid ethylene is much more opaque. These results must, however, be considered only as an approximation to the truth, and as generally confirmatory of the inferences Tyndall drew as to the relation between gases and liquids as absorbents of radiant heat.

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\* 'On the Refractive Indices of Liquid Nitrogen and Air By Professors Liveing and Dewar. Phil Mag 1893.

Instead of silvering the interior and exterior of the vacuum vessels, it is found convenient when using mercury vacua to leave a little excess of liquid mercury, in order that the act of filling the inner vessel with liquid air should cause a fine silvery deposit of the metal over the exterior surface of the inner vessel. In such a vessel liquid air or oxygen shows no signs of ebullition, the surface remains as quiet and still as if it was ordinary water. The supply of heat is cut down to less than four per cent. of what it is without exhaustion and silvering in good vacuum vessels. The result is that volatile liquids can be kept thirty times longer. Such vessels do not, however, maintain indefinitely the high standard of heat isolation they possess the first time they are used. After repeated use all vacuum vessels employed in the storage and manipulation of liquid air deteriorate. Illustrations of the appearance of such vessels are given in Figs. 1 and 2. The rapidity with which a space is saturated with mercury vapour (which we know exerts a pressure of about one-millionth of an atmosphere) is easily proved by simply filling a barometer in the usual way, and then instantly applying a sponge of liquid air to a portion of the glass surface of the Torricellian vacuum space, when a mercury mirror immediately deposits. It is important to know the amount of mercury deposited from a saturated atmosphere which is maintained (containing excess of liquid mercury) at the ordinary temperature, the condensation taking place when liquid air or oxygen is discharged into a vessel surrounded by such a Torricellian vacuum. If the deposit on the cooled bulb is allowed to take place for a given time, the outer vessel can then be broken and the amount of mercury which coated the bulb ascertained by weighing. Knowing the surface of the cooled bulb, the amount deposited per unit of area can be calculated. In this way it was found that in ten minutes 2 milligrams of mercury per square centimetre of surface was deposited. Considering that one-tenth of a milligram of mercury in the form of saturated vapour at the ordinary temperature corresponds to the volume of 1 litre, this proves that the equivalent weight of 20 litres had been condensed in the space of ten minutes. This plan of cooling a portion of the surface of a vessel by the application of a liquid air sponge, enables us to test our conclusions as to the amount of matter present in certain vacua. Here is a globe of the capacity of 1 litre. It has been filled with, presumably, nothing but the vapour of mercury, by boiling under exhaustion and subsequent removal of all excess of liquid. Such a flask ought to contain mercury in the gaseous state that would weigh rather less than one-tenth of a milligram, assuming the ordinary gaseous laws extend to pressures of less than one-millionth of an atmosphere. Now we know by electric deposition that one-tenth of a milligram of gold can be made to cover one square centimetre of surface with a fine metallic deposit. Considering the general similarity in the properties of mercury and gold, we should therefore anticipate that if all the mercury vapour could be frozen out of the litre flask it would also form a mirror about one square centimetre in area. But after one such mirror is deposited, the renewed application of a second

liquid air sponge to another portion of the surface would cause no visible deposit. This is exactly what takes place. If, however, two spheres, one much larger than the other, are joined together by means of a tube about 2 mill. in diameter and 50 mill. long, the whole space being a Torricellian vacuum (with some excess of mercury) then on decanting, the mercury may be transferred to the smaller sphere, as is represented in Fig. 3. Now if an air sponge is applied to a portion of the surface of the larger sphere, a mercury mirror instantly deposits, but on applying a new air sponge to another portion of the surface, no further mercury mirror is formed. The narrow glass tube prevents the excess of liquid mercury in the small bulb supplying vapour rapidly to the larger one, so that the local cooling to  $-180^{\circ}$  C. of a portion of the surface has practically condensed all the mercury in the larger space, although the small one is still filled with saturated vapour and a free communication exists between them. If while in this condition the small bulb is inclined so as to allow a drop of liquid mercury to fall into the lower side of the large bulb, which has not been cooled, instant deposition of mercury takes place on the liquid air cooled portion of the upper surface. Under very small pressure of vapour, therefore, equalisation of pressure of two bulbs communicating by a narrow tube is a very slow process. There are cases, however, in which the application of a sponge of liquid air to the surface of a vessel causes no visible deposit, and yet the inference is that something has been condensed. The best arrangement to show this effect is to select highly exhausted vacuum tubes containing phosphorescent materials like alumina and other minerals, and to arrange the induction coil spark gap of a little greater resistance than the vacuum tube. On starting the coil the current passes solely by the vacuum tube, but immediately the liquid air sponge cools a portion of the surface of the bulb, the discharge shifts to the air gap. During the cooling the phosphorescence of the glass tube is greatly increased, but finally the resistance may become so great that all discharge in the vacuum tube ceases. Some old tubes belonging to the late Dr. de la Rue have given visible deposits near the electrodes, and in many the diameter and distribution of the striæ are materially changed during the local cooling to  $-190^{\circ}$  C. When large vessels containing nothing but mercury or iodine vapour as a residuum of the vacuum space are rubbed with a cotton wool sponge of liquid air in a dark room, luminous glows filling the vessel take place occasionally, or bright flashes of light which enable the shape of the vessel to be seen. The ordinary mercury vacuum vessels show the same phenomena, which is doubtless due to electric discharges caused by friction and cooling.

The optical properties of bodies cooled to the temperature of boiling liquid air will require long and patient investigation. An interesting fact easily observed is the marked change in colour of various bodies. Thus, for instance, oxide, sulphide, iodide of mercury, bichromate of potash, all become yellow or orange; while nitrate of uranium and the double chloride of platinum and ammonium become

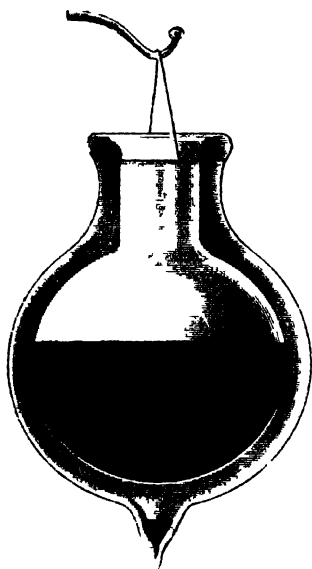


Fig. 1



Fig. 2

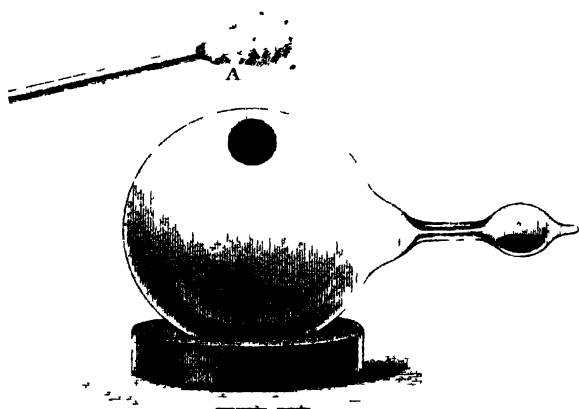


Fig. 3.

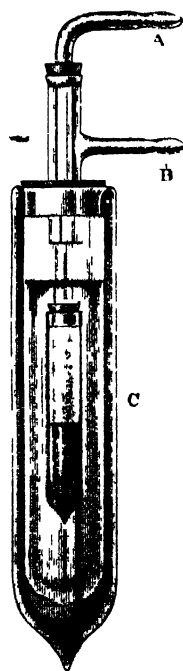


Fig. 4.

white. Chromic acid, dilute solution of iodine in alcohol, strong solutions of ferric chloride and other coloured solutions become greatly changed. Such facts are sufficient to prove that the specific absorption of many substances undergoes great changes at the temperature of  $-190^{\circ}\text{C}$ .

The tranquil atmosphere of air above the surface of the liquid in cylindrical or spherical vacuum vessels is a convenient place to cool very fragile bodies. During the slow ebullition of the fluid, gas between  $-190^{\circ}\text{C}$ . and  $-180^{\circ}\text{C}$ . is given off, which has three times the density of ordinary air, and which falls slowly over the mouth of the vessel in a heavy stream. On dipping into this atmosphere small soap bubbles, they contract rapidly and then freeze. If a soap film is made on a circle of thin wire about 2 inches in diameter, and allowed to stand until it shows the various orders of coloured bands, and is then carefully dipped into the cool air, it freezes, showing all the original colours. The black band is, however, always broken. Speaking of films, an interesting experiment may be made with a thin stretched sheet of india-rubber, such as is used for making balloons. It is well known that stretched india-rubber contracts when heated and expands when cooled. Now this can be shown very easily by covering a glass funnel or the end of a cylindrical vessel with a stretched sheet of rubber as thin as the walls of balloons. Such a surface is quite flat and fairly transparent. If a sponge of liquid air is drawn across the surface, the course is marked by a series of wrinkles, due to the temporary expansion of the rubber caused by the extreme cold. The sheet of rubber being extremely thin, soon regains the ordinary temperature, and the surface then is as flat and tense as before. During the continuous motion of the cotton wool liquid air sponge over the rubber surface, it is followed by wave-like depressions which disappear almost as quickly as they are formed. The elasticity of india-rubber, after cooling to  $-182^{\circ}\text{C}$ . and reheating, seems unimpaired.

Organic substances that only become solid at very low temperatures may be divided into two classes: those which crystallise, and those which form glasses. Thus bisulphide of carbon, tetrachloride of carbon, methyl alcohol, hydride of amyl, all form crystals, whereas ethyl alcohol, amyl alcohol, turpentine, ethyl nitrate, chinoline, picolin, are glass-like. If a few drops of bisulphide of carbon are added to alcohol and the mixture cooled to  $-180^{\circ}\text{C}$ ., a white solid emulsion is formed, whereas the addition of tetrachloride of carbon to the alcohol resulted in the production of a clear solid without any separation. In the same way pure methyl alcohol crystallises easily, but the addition of a few drops of ethyl alcohol prevents crystallisation and causes a glass to be formed. Thus the examination of the behaviour of organic bodies at low temperatures may be a fruitful means of organic investigation.

For many purposes of investigation it is necessary to keep liquid air without evaporation. This is readily done by the use of two vacuum test-tubes, fitting freely one inside the other, arranged as in Fig. 4.

The smaller one is filled with liquid air, and after the insertion of an india-rubber stopper and glass tube, is completely immersed in liquid air contained in the larger vacuum vessel. In the figure the tube A connects with the inner vacuum tube and B with the outer. As the latter receives all the radiant and conducted heat, air is continuously boiling off through the tube B, but as the supply of heat is effectually cut off from the inner vacuum vessel, also containing liquid air, no air distils through tube A. This is the most convenient arrangement to use for the production of solid air. For this purpose B is connected with an air pump until the pressure is reduced to about  $\frac{1}{2}$  inch, and therefore the temperature about  $-200^{\circ}\text{C}$ . Then a good air pump is put on to the inner vessel of liquid air (containing oxygen and nitrogen in the normal proportion of oxygen and nitrogen), by means of the tube A, while maintaining constantly the exhaustion in the outer vessel. In a short time the air in the inner vessel solidifies to a transparent jelly-like mass.

The same principle is used when the latent and specific heats have to be determined. Fig. 5 shows the general plan of the apparatus. Now a definite quantity of heat has to be conveyed into the inner vacuum vessel containing liquid air, with the object of finding the weight of liquid that distils off, on the one hand, or the elevation of temperature in the liquid that takes place on the other. For the purpose of adding a given quantity of heat it is convenient in some cases to use mercury (as represented in the figure), or to lower a piece of platinum or silver, or even glass, into the inner vessel: each unit of heat supplied evaporates a definite amount of air, which is readily ascertained by collecting the gas which comes off during the heat conveyance. In Fig. 5, A is the mercury, C the inner vessel of liquid air, D a three-way stop-cock, F a tube for collecting the air given off; E is a barometric tube for observing the pressure when the inner vessel is exhausted. In a latent heat determination all that is necessary is to weigh the mercury added and to measure the amount of air by volume which has distilled from the liquid state. If the specific heat of the liquid is wanted, then the inner vessel is exhausted (as well as the outer) through the tube F to about  $\frac{1}{2}$  inch pressure, and the three-way stop-cock turned so as to shut off F and connect the inner vessel with the manometer E. Mercury is now dropped into the inner vessel until the manometer rises to the atmospheric pressure or the liquid reaches its boiling point under atmospheric pressure. Care must be taken to prevent the drops of mercury falling exactly in the same place, otherwise a mercury stalagmite grows up rapidly through the liquid, vitiating the results. Another objection to the use of mercury arises from the drops causing the rebound of small liquid air drops, which strike the cork and get evaporated away from the main body of liquid. The amount of mercury added conveys the necessary amount of heat needed to raise the given amount of liquid from its boiling point under  $\frac{1}{2}$  inch pressure to its boiling point under 30 inches. The relative pressures give the temperature range, and the weight of liquid air or other



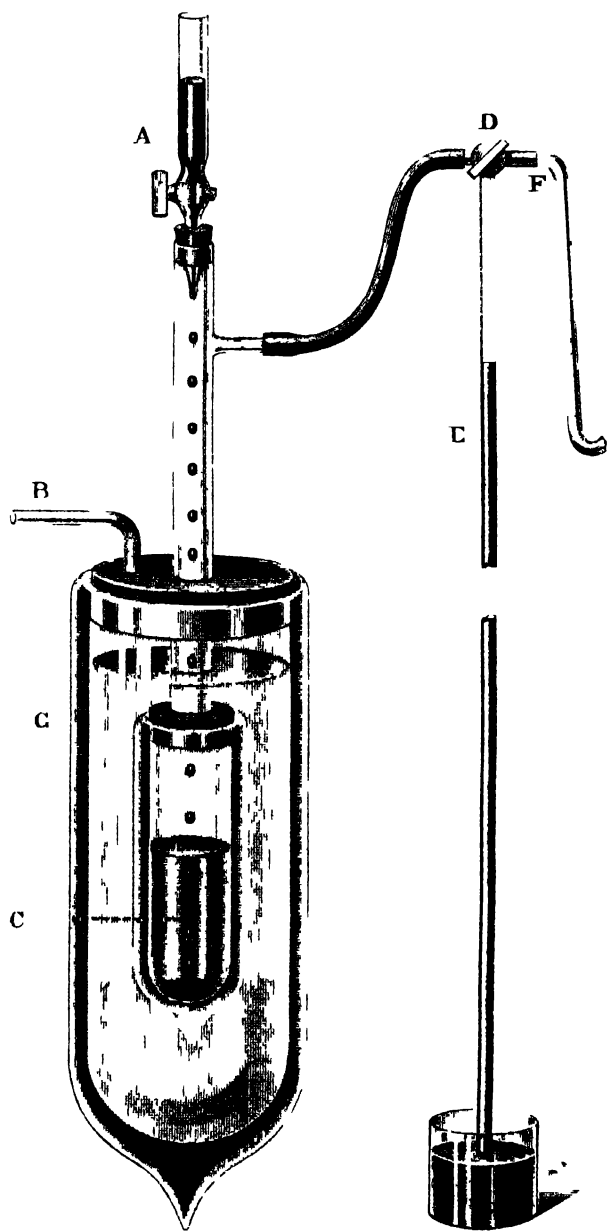


Fig. 5

gas under observation is easily ascertained, together with the weight of mercury added. In this way the latent heat of liquid oxygen at its boiling point is about 80 units, and the mean specific heat between  $-198^{\circ}$  and  $-182^{\circ}$  is 0.39.

Seeing that the most powerful chemical affinities are in abeyance at very low temperatures, it is a matter of great interest to ascertain what change comes over the physical force we name cohesion. Here we are dealing with the molecular forces which are effective in uniting together the particles of solid bodies, in contrast to the force we name chemical attraction, which exists most characteristically between dissimilar molecules. Both are alike in this respect, that they are insensible at sensible distances. If we accept the theory of matter which regards finite heterogeneity of the most homogeneous bodies as proved, then Lord Kelvin has shown that gravitation alone would account for the so called cohesive forces. Thus, he says ('Popular Lectures,' vol. i. page 60): "But if we take into account the heterogeneous distribution of density essential to any molecular theory of matter, we readily see that it alone is sufficient to intensify the force of gravitation between two bodies placed extremely close to one another, or between two parts of one body, and therefore that cohesion may be accounted for, without assuming any other force than that of gravitation, or any other law than the Newtonian." Another view of the cohesive forces is taken by Mr. S. Tolver Preston, in his work entitled 'Physics of the Ether,' page 64. He says, "The phenomena of 'cohesion,' 'chemical union,' &c., or the general phenomena of the aggregation of molecules, being dependent on the molecular vibrations as a physical cause, it would therefore be reasonable to conclude that variation of vibrating energy (variation of 'temperature') would have a most marked influence on these phenomena, as is found to be the fact. Further, since when a physical cause ceases to exist the effect also ceases, it follows that at the absolute zero of temperature (absence of vibrating energy) the general phenomena of 'cohesion,' including the aggregation of molecules in chemical union, would cease to exist." If this theory is pressed so as to include the gaseous state, then at the temperature of  $-274^{\circ}$  C. we may imagine the particles reduced to an incoherent layer of dust or powder. The experimental facts do not, however, warrant this conclusion, seeing that at the lowest temperature reached, which is about  $-210^{\circ}$  C., air remains a transparent jelly. That a low temperature causes profound changes in the elastic constants of a metallic body is most easily shown by placing a rod of fusible metal in liquid air, and comparing the deflection produced by a weight when the rod is supported at one or both ends before and after cooling.

The Young modulus is increased to between four and five times its amount at ordinary temperatures. In the same way, the rigidity modulus can be shown to be greatly changed by cooling a spiral spring made of fusible metal wire. Such a spring at the ordinary temperature is quickly drawn out into a straight wire, by attempting

to make it support an ounce weight. The same spiral, cooled to  $-182^{\circ}\text{C.}$ , will support a couple of pounds, and will vibrate like a steel spring so long as it is cool. In the same way, a bell or tuning fork of fusible metal gives a distinct metallic ring at  $-180^{\circ}$ . If two tuning-forks are taken of identical pitch, and one cooled to  $-182^{\circ}$ , then on simultaneously striking them beats are very distinctly heard. The simplest plan of getting some idea of the change in the cohesive force at low temperatures, is to ascertain the tenacity or breaking stress of the metals and alloys under such conditions, and to compare such results with similar experiments made at the ordinary temperature with the same metallic samples, using the same apparatus. In this way the comparative values are reliable. The only difficulty is the large quantity of liquid air or oxygen required to cool the steel supports of the wires, which have to be broken. Seeing that wires less than  $\frac{1}{10}$  inch in diameter are unreliable, good strong rigid steel supports are needed, and as these have to be cooled each time a wire is broken, the experiments involve large quantities (gallons) of liquid air and oxygen. Further, as not less than three, and in many cases six experiments must be made with each sample of wire, and the stress in each case can only be applied slowly, work of this kind extends over long periods of time, and this means increased waste of liquid gases. Fig. 6 shows the general plan of the part of the testing machine which supports the wires which have to be broken. In the figure, A is the steel rod which is connected to the multiplying levers, the stress being gradually increased as usual by running in water into a vessel hung from the long end of the lever; C is the wire to be tested, B is an arrangement which measures roughly the extension of the wire, and D is a large silvered vacuum vessel holding the liquid oxygen. This latter vessel must be large, in order to avoid any part of the supports of the wire coming into contact with the sides, otherwise the shock of the wire on breaking shatters the vacuum bulb. The rupture must be made while the wire is immersed in the liquid oxygen, and the whole of the supports thoroughly cooled down. The wires must be caught in long V-shaped grooves made in the steel supports in order to avoid slipping, and change in the cross section of the wire. As a rule, the wires used were  $\frac{1}{10}$  inch in diameter and 2 inches long. The following table gives the mean results of a large number of experiments:—

TABLE I.—BREAKING STRESS IN POUNDS OF METALLIC WIRES  
0.098 INCHES DIAMETER.

	15° C.	$-182^{\circ}\text{C.}$		15° C.	$-182^{\circ}\text{C.}$
Steel (soft) ..	120	700	German silver	170	600
Iron .. ..	320	670	Gold .. ..	255	340
Copper .. ..	200	300	Silver .. ..	330	420
Brass .. ..	310	440			

An inspection of this table proves that all the common metals and alloys increase in tenacity at low temperatures: thus iron has doubled its breaking stress, and the other metals and alloys are all increased from a third to a half the normal amount. This increase

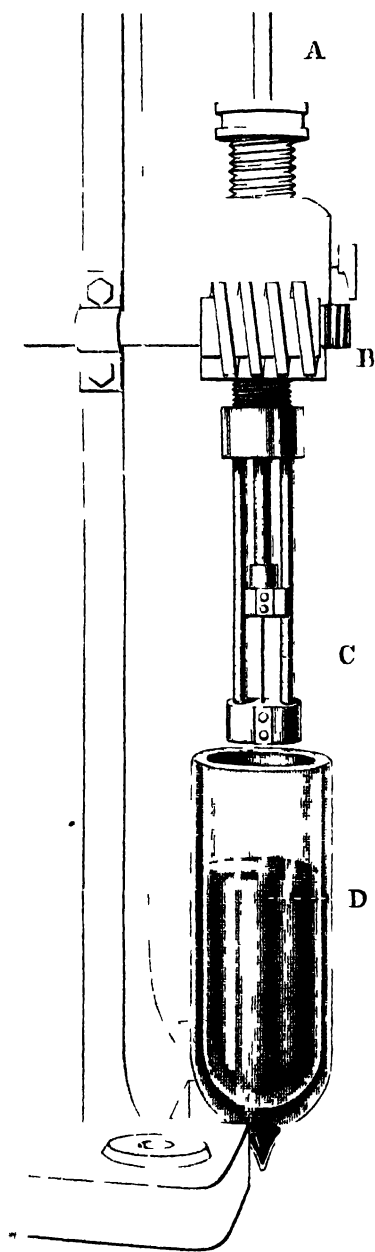


Fig 6

of strength is solely due to the low temperature, and persists only during its continuance. Wires that have been cooled to the temperature of  $-182^{\circ}\text{C}$ . and allowed to regain the ordinary temperature, are in no way changed as regards their breaking stress.

A second series of experiments were made with a set of cast test pieces of metals and alloys. The test pieces, all cast in the same mould, were 2 inches long with  $\frac{1}{2}$  inch spherical ends, the cylindrical portion being  $\frac{1}{8}$  inch diameter. The spherical ends of the test pieces rested in similar cavities made in a special set of steel supports that fitted on to the testing machine. Crystalline metals give castings that are far from uniform one with another, and it is very difficult to get even comparable results with metals like zinc, bismuth and antimony. The following table gives the experimental results:—

TABLE II.—BREAKING STRESS IN POUNDS OF CAST METALLIC TEST PIECES.  
DIAMETER OF ROD 0.2 INCH.

	15° C	−182° C.		15° C.	−182° C
Tin .. ..	200	390	Bismuth .. ..	60	30
Lead .. ..	77	170	Antimony .. ..	61	30
Zinc .. ..	35	26	Solder .. ..	300	645
Mercury ..	0	31	Fusible metal (Woods)	140	450

It will be noted that in this list the breaking stress, by cooling to  $-182^{\circ}\text{C}$ ., has been increased to three times its usual value in the case of fusible metal, and to twice its usual value in the case of tin, lead and solder. The results with zinc, bismuth and antimony are exceptional, seeing they appear to be diminished in tenacity. This, however, may be only apparent, because the stresses set up in cooling such highly crystalline bodies probably weaken some set of cleavage planes so that rupture is then comparatively easy. In any case it must be admitted that no reliance can be placed on the tenacity of highly crystalline metals. The breaking stress of mercury is interesting, and turns out to be at  $-182^{\circ}\text{C}$ . nearly half that of lead at the ordinary temperatures. The percentage elongation is not given in the foregoing tables, simply because the value of such measurements is of little importance when such short pieces of the metals are under observation. The general results of such observations are, however, interesting: thus, lead and tin at ordinary temperatures elongate before breaking about the same amount, whereas if tin is cooled to  $-182^{\circ}\text{C}$ . it hardly shows any extension, and lead under such conditions shows no change, stretching as much at  $-182^{\circ}$  as at  $15^{\circ}\text{C}$ . Solder and fusible metal stretch less, and the cross section of the break is much less at  $-182^{\circ}$  than at  $15^{\circ}\text{C}$ . The above experiments can only be considered as preliminary to a more elaborate investigation of the actual variation of the elastic constants at low temperatures. It will require complex experimental arrangements to get reliable measurements of the Young modulus and the rigidity modulus at the temperature of boiling liquid air. In the case of fusible metal, a first attempt to compare the ratio of the Young modulus at  $15^{\circ}$  and

—  $182^{\circ}$  with the ratio of the rigidity modulus between the same limits of temperature, has resulted in finding that both constants are increased in the same proportion. From this it would follow that the resistance to compression of the substance at  $-182^{\circ}$  C. must be increased in a similar ratio. The comparative behaviour of strong steel spirals at  $15^{\circ}$  C. and  $-182^{\circ}$  as to their elongation on the repeated addition of the same load was a subject examined on several occasions. The most careful comparison of such spirals, however, revealed no measurable differences in their elongation between the ordinary temperature and that of boiling oxygen. This may be due to the want of sufficient sensibility in the testing machine when applied to such delicate experiments. In the meantime it is reasonable to conclude that the rigidity modulus of very hard steel is not much changed by cooling it to  $-182^{\circ}$  C. If balls of iron, tin, lead or ivory are cooled to  $-182^{\circ}$  C. and dropped from a fixed height on a massive iron anvil the elastic rebound is markedly increased in all cases. The flat distortion surface produced on the lead sphere after impact is only one-third the diameter of the circular surface produced at the ordinary temperature when the lead ball falls from the same height.

The examination of the magnetic condition of matter at low temperatures is a subject of great interest and offers a wide field for investigation. In a former lecture the magnetic properties of liquid oxygen and air were discussed. Owing to the experimental difficulties, accurate quantitative measurements of the permeability have not yet been successful. Faraday was the first experimenter who examined the magnetic condition of matter at the lowest temperature that could be commanded in his time, viz. about  $-110^{\circ}$  C. He did not succeed in making any substance which was non-magnetic at ordinary temperatures assume the magnetic state at the lowest temperature of the solid carbonic acid ether bath in vacuo. Later experimenters have directed their attention more especially to the action of high temperatures on magnetism, and the work of Professors Hopkinson and Ewing in this field of research is well known. Professor Trowbridge examined the effect of a temperature of  $-80^{\circ}$  C. on a permanent magnet, and came to the conclusion that the magnetic moment was diminished by about 50 per cent. Professor Ewing found that an increase of temperature of  $150^{\circ}$  C. above  $10^{\circ}$ , caused a reduction of the magnetic moment of a bar magnet by about 40 per cent., and that the magnet on cooling recovered its original state. This result would lead us to expect that if the same law is followed below the melting point of ice as Ewing found above it, then a bar magnet cooled to  $-182^{\circ}$  C. ought to gain in magnetic moment something like 30 to 50 per cent. The experiment of Professor Trowbridge is, however, apparently opposed to such an inference. It appears, however, that Professor Trowbridge cooled a magnet that had not reached a constant state (that is to say, one that on heating would not have completely recovered its magnetisation on cooling), because after the magnet had been cooled to  $-80^{\circ}$  on regaining the ordinary tempera-

ture, it had lost 50 per cent. of its original magnetic moment. Such a magnet would apparently diminish in magnetic moment on cooling and heating the first time the action was examined, but a repetition of the process when the action of magnetisation and temperature were strictly reversible might lead to an opposite conclusion. To settle this question a series of experiments on the magnetic moment of small magnets cooled to  $-182^{\circ}$  were carried out. Small magnets from half an inch to an inch in length were made of watch-spring or steel wire and were either used separately or in bundles; they were fixed rigidly in a block of wood by means of copper staples, and in this condition were easily clamped firmly in the field of a magnetometer. The cooling was effected by applying a cotton-wool sponge of liquid air. The relative deviations of the magnetometer are proportional to the magnetic moment of the magnet under the respective conditions of  $+15^{\circ}$  and  $-182^{\circ}$  C. After the first cooling the magnet is allowed to regain the ordinary temperature, and the operation of cooling and heating is repeated three or four times. The following table gives some of the results, and these may be taken as typical of a large additional number unrecorded.

CHANGE OF THE MAGNETIC MOMENTS OF PERMANENT MAGNETS AT  $+15^{\circ}$  AND  $-182^{\circ}$  C. per cent. of the value at the beginning of each cycle, which is always  $15^{\circ}$ .

								$-182^{\circ}$ C.	$+15^{\circ}$ C.
(1) Hard steel, 0.5 inches long and 0.4 inches diameter.									
	First Cycle	..	..	..	..	..	..	+ 0	- 30
	Second "	..	..	..	..	..	..	+ 33	- 5
	Third "	..	..	..	..	..	..	+ 36	0
(2) Soft steel.									
	First Cycle	..	..	..	..	..	..	+ 12	- 28
	Second "	..	..	..	..	..	..	+ 51	0
	Third "	..	..	..	..	..	..	+ 51	0
(3) Hard steel, 1.03 inches long, 0.4 inches diameter.									
	First Cycle	..	..	..	..	..	..	- 24	- 43.4
	Second "	..	..	..	..	..	..	+ 23	0
	Third "	..	..	..	..	..	..	+ 23	0
(4) Nine steel wires in bundle									
	First Cycle	..	..	..	..	..	..	+ 12.5	+ 3
	Second "	..	..	..	..	..	..	+ 38	- 2
	Third "	..	..	..	..	..	..	+ 32	0
Tested four days after.									
	First Cycle	..	..	..	..	..	..	+ 50	0

If the experiment marked (1) is examined we find cooling to  $-182^{\circ}$ , in the first cycle produced no change of magnetic moment, but that on heating to  $+15^{\circ}$  C. the magnet had lost 30 per cent. of the original strength. In the second cycle cooling increased the magnetic strength of the magnet, in the condition in which it is left after the first cooling by 33 per cent., and heating diminished it by 5 per cent.; whereas in the third cycle cooling showed 36 per cent. increase and no loss in heating. It was only after three alterations of temperatures from  $+15^{\circ}$  to  $-182^{\circ}$  C. that the magnet reached a steady

condition. In experiment (3) the first cooling shows a loss of 24 per cent., while in experiment (4) the first cooling shows a gain of 12½ per cent.

It is clear, therefore, that according to these experiments, every magnet has individual characteristics that may either result in no change on cooling or the addition or subtraction of from 12 to 24 per cent. in the magnetic strength. All the experiments, however, show that a repetition of the cycle of heating and cooling brings the magnet to a steady state, in which cooling always causes increase in the magnetic strength of from 30 to 50 per cent., and the re-heating brings about no loss in the original magnetic moment. Such a marked alteration of magnetic strength might be used as a thermometer in low temperature research, and it is my intention to extend the inquiry to the lowest temperature that can be reached by the evaporation of nitrogen in vacua. A simple mode of showing the sudden alteration of magnetic strength on cooling, is to surround a permanent magnet made up of a bundle of steel wires with a coil of copper wire, leaving the ends of the magnet to project so that they can be dipped in liquid air. When the copper wires are attached to a galvanometer, and one of the ends of the magnet cooled, an induced electrical current occurs, due to the sudden magnetic change. Accurate observations must be made on the permeability and susceptibility of the magnetic metals at the temperature of boiling liquid air, and the above results are an indirect guarantee that this field of investigation will be fruitful in new scientific facts.

This lecture has already covered a very wide field. It is easy to put into a Friday evening discourse the work of a year. Members and friends have chiefly contributed to the Research Fund, which has enabled the Institution to extend the experimental plant needed for the prosecution of research in this field of inquiry, and they have strong claims to learn, in the first instance, the results of the general laboratory work. My object has been to illustrate the scientific uses of liquid air. To do this with any satisfaction requires what may be called a good deal of scientific prospecting. It is one thing to discover where the ore lies, it is another thing to produce the refined metal. Investigations on the properties of matter at the temperature of boiling liquid air, must be in the first instance rather qualitative than rigidly quantitative. In my opinion scientific progress is best served by conducting the inquiry on these lines. It will be easy to refine later on.

I have to acknowledge the great assistance I have received in the conduct of these experiments, from my excellent chief assistant Mr. Robert Lennox, and I must also express commendation of the way Mr. Heath has helped in the work.

[J. D.]



Friday, March 16, 1894.

THE RIGHT HON. EARL PERCY, F.S.A. Vice-President,  
in the Chair.

THE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. M.R.I.

PROFESSOR OF NATURAL PHILOSOPHY, R.I.

*The Scientific Work of Tyndall.*

It is fitting that the present season should not pass without a reference on these evenings to the work of him whose tragic death a few months since was felt as a personal grief and loss by every member of the Royal Institution. With much diffidence I have undertaken the task to-night, wishing that it had fallen to one better qualified by long and intimate acquaintance to do justice to the theme. For Tyndall was a personality of exceeding interest. He exercised an often magical charm upon those with whom he was closely associated, but when his opposition was aroused he showed himself a keen controversialist. My subject of to-night is but half the story.

Even the strictest devotion of the time at my disposal to a survey of the scientific work of Tyndall will not allow of more than a very imperfect and fragmentary treatment. During his thirty years of labour within these walls he ranged over a vast field, and accumulated results of a very varied character, important not only to the cultivators of the physical sciences, but also to the biologist. All that I can hope to do is to bring back to your recollection the more salient points of his work, and to illustrate them where possible by experiments of his own devising.

In looking through the catalogue of scientific papers issued by the Royal Society, one of the first entries under the name of Tyndall relates to a matter comparatively simple, but still of some interest. It has been noticed that when a jet of liquid is allowed to play into a receiving vessel, a good deal of air is sometimes carried down with it, while at other times this does not happen. The matter was examined experimentally by Tyndall, and he found that it was closely connected with the peculiar transformation undergone by a jet of liquid which had been previously investigated by Savart. A jet as it issues from the nozzle is at first cylindrical, but after a time it becomes what the physiologists call *varicose*; it swells in some places and contracts in others. This effect becomes more exaggerated as the jet descends, until the swellings separate into distinct drops, which follow one another in single file. Savart showed that under the influence of

vibration the resolution into drops takes place more rapidly, so that the place of resolution travels up closer to the nozzle.

Tyndall's observation was that the carrying down of air required a jet already resolved into drops when it strikes the liquid. I hope to be able to show you the experiment by projection upon the screen. At the present moment the jet is striking the water in the tank previous to resolution into drops, and is therefore carrying down no air. If I operate on the nozzle with a vibrating tuning-fork, the resolution occurs earlier, and the drops now carry down with them a considerable quantity of air.

Among the earlier of Tyndall's papers are some relating to ice, a subject which attracted him much, probably from his mountaineering experiences. About the time of which I am speaking Faraday made interesting observations upon a peculiar behaviour of ice, afterwards called by the name of regelation. He found that if two pieces of ice were brought into contact they stuck or froze together. The pressure required to produce this effect need not be more than exceedingly small. Tyndall found that if fragments of ice are squeezed they pack themselves into a continuous mass. We have here some small ice in a mould, where it can be subjected to a powerful squeeze. The ice under this operation will be regelated, and a mass obtained which may appear almost transparent, and as if it had never been fractured at all. The flow of glaciers has been attributed to this action, the fractures which the stresses produce being mended again by regelation. I should say, perhaps, that the question of glacier motion presents difficulties not yet wholly explained. There can be no doubt, however, that regelation plays an important part.

Another question treated by Tyndall is the manner in which ice first begins to melt under the action of a beam of light passing into it from an electric lamp. Ice usually melts by conducted heat, which reaches first the outside layers. But if we employ a beam from an electric lamp, the heat will reach the ice not only outside but internally, and the melting will begin at certain points in the interior. Here we have a slab of ice which we project upon the screen. We see that the melting begins at certain points, which develop a crystallised appearance resembling flowers. They are points in the interior of the ice, not upon the surface. Tyndall found that when the ice gives way at these internal points there is a formation of apparently empty space. He carefully melted under water such a piece of ice, and found that when the cavity was melted out there was no escape of air, proving that the cavity was really vacuous.

Various speculations have been made as to the cause of this internal melting at definite points, but here again I am not sure if the difficulty has been altogether removed. One point of importance brought out by Tyndall relates to the plane of the flowers. It is parallel to the direction in which the ice originally froze, that is, parallel to the original surface of the water from which it was formed.

I must not dwell further upon isolated questions, however interesting; but will pass on at once to our main subject, which may be divided into three distinct parts, relating namely to heat, especially dark radiation, sound, and the behaviour of small particles, such as compose dust, whether of living or dead matter.

The earlier publications of Tyndall on the subject of heat are for the most part embodied in his work entitled 'Heat as a Mode of Motion.' This book has fascinated many readers. I could name more than one now distinguished physicists who drew their first scientific nutriment from it. At the time of its appearance the law of the equivalence of heat and work was quite recently established by the labours of Mayer and Joule, and had taken firm hold of the minds of scientific men; and a great part of Tyndall's book may be considered to be inspired by and founded upon this first law of thermodynamics. At the time of publication of Joule's labours, however, there seems to have been a considerable body of hostile opinion, favourable to the now obsolete notion that heat is a distinct entity called caloric. Looking back, it is a little difficult to find out who were responsible for this reception of the theory of caloric. Perhaps it was rather the popular writers of the time than the first scientific authorities. A scientific worker, especially if he devotes himself to original work, has not time to examine for himself all questions, even those relating to his own department, but must take something on trust from others whom he regards as authorities. One might say that a knowledge of science, like a knowledge of law, consists in knowing where to look for it. But even this kind of knowledge is not always easy to obtain. It is only by experience that one can find out who are most entitled to confidence. It is difficult now to understand the hesitation that was shown in fully accepting the doctrine that heat is a mode of motion, for all the great authorities, especially in England, seem to have favoured it. Not to mention Newton and Cavendish, we have Rumford making almost conclusive experiments in its support, Davy accepting it, and Young, who was hardly ever wrong, speaking of the antagonistic theory almost with contempt. On the Continent perhaps, and especially among the French school of chemists and physicists, caloric had more influential support.

As has been said, a great part, though not the whole of Tyndall's work was devoted to the new doctrine. Much relates to other matters, such as radiant heat. Objection has been taken to this phrase, not altogether without reason; for it may be said that when heat it is not radiant, and while radiant it is not heat. The term dark radiation, or dark radiance as Newcombe calls it, is preferable, and was often used by Tyndall. If we analyse, as Newton did, the components of light, we find that only certain parts are visible. The invisible parts produce, however, as great, or greater, effects in other ways than do the visible parts. The heating effect, for example, is vastly greater in the invisible region than in the visible. One of the experiments that Tyndall devised in order to illustrate this fact I

hope now to repeat. He found that it was possible by means of a solution of iodine in bisulphide of carbon to isolate the invisible rays. This solution is opaque to light; even the sun could not be seen through it; but it is very fairly transparent to the invisible ultra-red radiation. By means of a concave reflector I concentrate the rays from an arc lamp. In their path is inserted the opaque solution, but in the focus of invisible radiation the heat developed is sufficient to cause the inflammation of a piece of gun-cotton.

Tyndall varied this beautiful experiment in many ways. By raising to incandescence a piece of platinum foil, he illustrated the transformation of invisible into visible radiation.

The most important work, however, that we owe to Tyndall in connection with heat is the investigation of the absorption by gaseous bodies of invisible radiation. Melloni had examined the behaviour of solid and liquid bodies, but not of gaseous. He found that transparent bodies like glass might be very opaque to invisible radiation. Thus, as we all know, a glass screen will keep off the heat of a fire, while if we wish to protect ourselves from the sun, the glass screen would be useless. On the other hand rock salt freely transmitted invisible radiation. But nothing had been done on the subject of gaseous absorption, when Tyndall attacked this very difficult problem. Some of his results are shown in the accompanying table. The absorption of the ordinary non-condensable, or rather, not easily condensable gases—for we must not talk of non-condensable gases now, least of all in this place—the absorption of these gases is very small; but when we pass to the more compound gases, such as nitric oxide, we find the absorption much greater—and in the case of olefiant gas we see that the absorbing power is as much as 6000 times that of the ordinary gases.

Relative Absorption at  
1 inch Pressure.

Air .. .. .	1
Oxygen .. .. .	1
Nitrogen .. .. .	1
Hydrogen .. .. .	1
Carbonic acid .. .. .	972
Nitric oxide .. .. .	1590
Ammonia .. .. .	5460
Olefiant gas .. .. .	6030

There is one substance as to which there has been a great diversity of opinion—aqueous vapour. Tyndall found that aqueous vapour exercises a strong power of absorption—strong relatively to that of the air in which it is contained. This is of course a question of great importance, especially in relation to meteorology. Tyndall's conclusions were vehemently contested by many of the authorities of the time, among whom was Magnus, the celebrated physicist of Berlin. With a view to this lecture I have gone somewhat carefully into this question, and I have been greatly impressed by the care and skill showed by Tyndall, even in his earlier experiments upon this subject. He

was at once sanguine and sceptical—a combination necessary for success in any branch of science. The experimentalist who is not sceptical will be led away on a false tack and accept conclusions which he would find it necessary to reject were he to pursue the matter further; if not sanguine, he will be discouraged altogether by the difficulties encountered in his earlier efforts, and so arrive at no conclusion at all. One criticism, however, may be made. Tyndall did not at first describe with sufficient detail the method and the precautions which he used. There was a want of that precise information necessary to allow another to follow in his steps. Perhaps this may have been due to his literary instinct, which made him averse from overloading his pages with technical experimental details.

The controversy above referred to I think we may now consider to be closed. Nobody now doubts the absorbing power of aqueous vapour. Indeed the question seems to have entered upon a new phase; for in a recent number of Wiedemann's 'Annalen,' Paschen investigates the precise position in the spectrum of the rays which are absorbed by aqueous vapour.

I cannot attempt to show you here any of the early experiments on the absorption of vapours. But some years later Tyndall contrived an experiment, which will allow of reproduction. It is founded on some observations of Graham Bell, who discovered that various bodies became sonorous when exposed to intermittent radiation.

The radiation is supplied from incandescent lime, and is focussed by a concave reflector. In the path of the rays is a revolving wheel provided with projecting teeth. When a tooth intervenes, the radiation is stopped; but in the interval between the teeth the radiation passes through, and falls upon any object held at the focus. The object in this case is a small glass bulb containing a few drops of ether, and communicating with the ear by a rubber tube. Under the operation of the intermittent radiation the ether vapour expands and contracts; in other words a vibration is established, and a sound is heard by the observer. But if the vapour were absolutely diathermanous, no sound would be heard.

I have repeated the experiment of Tyndall which allowed him to distinguish between the behaviour of ordinary air and dry air. If, dispensing with ether, we fill the bulb with air in the ordinary moist state, a sound is heard with perfect distinctness, but if we drop in a little sulphuric acid, so as to dry the air, the sound disappears.

According to the law of exchanges, absorption is connected with radiation; so that while hydrogen or oxygen do not radiate, from ammonia we might expect to get considerable radiation. In the following experiment I aim at showing that the radiation of hot coal gas exceeds the radiation of equally hot air.

The face of the thermopile, protected by screens from the ball itself, is exposed to the radiation from the heated air which rises from a hot copper ball. The effect is manifested by the light reflected

from a galvanometer mirror. When we replace the air by a stream of coal gas, the galvanometer indicates an augmentation of heat, so that we have before us a demonstration that coal gas when heated does radiate more than equally hot air, from which we conclude that it would exercise more absorption than air.

I come now to the second division of my subject, that relating to Sound. Tyndall, as you know, wrote a book on Sound, founded on lectures delivered in this place. Many interesting and original discoveries are there embodied. One that I have been especially interested in myself, is on the subject of sensitive flames. Professor Leconte in America made the first observations at an amateur concert, but it was Tyndall who introduced the remarkable high-pressure flame now before you. It issues from a pin-hole burner, and the sensitiveness is entirely a question of the pressure at which the gas is supplied. Tyndall describes the phenomenon by saying that the flame under the influence of a high pressure is like something on the edge of a precipice. If left alone, it will maintain itself; but under the slightest touch it will be pushed over. The gas at high pressure will, if undisturbed, burn steadily and erect, but if a hiss is made in its neighbourhood it becomes at once unsteady, and ducks down. A very high sound is necessary. Even a whistle, as you see, does not act. Smooth pure sounds are practically without effect unless of very high pitch.

I will illustrate the importance of the flame as a means of investigation by an experiment in the diffraction of sound. I have here a source of sound, but of pitch so high as to be inaudible. The waves impinge perpendicularly upon a circular disc of plate glass. Behind the disc there is a sound shadow, and you might expect that the shadow would be most complete at the centre. But this is not so. When the burner occupies this position the flame flares; but when by a slight motion of the disc the position of the flame is made eccentric, the existence of the shadow is manifested by the recovery of the flame. At the centre the intensity of sound is the same as if no obstacle were interposed.

The optical analogue of the above experiment was made at the suggestion of Poisson, who had deduced the result theoretically, but considered it so unlikely that he regarded it as an objection to the undulatory theory of light. Now, I need hardly say, it is regarded as a beautiful confirmation.

It is of importance to prove that the flame is not of the essence of the matter, that there is no need to have a flame, or to ignite it at the burner. Thus, it is quite possible to have a jet of gas so arranged that ignition does not occur until the jet has lost its sensitiveness. The sensitive part is that quite close to the nozzle, and the flame is only an indicator. But it is not necessary to have any kind of flame at all. Tyndall made observations on smoke-jets, showing that a jet of air can be made sensitive to sound. The difficulty is to see it, and

to operate successfully upon it; because, as Tyndall soon found, a smoke-jet is much more difficult to deal with than flames, and is sensitive to much graver sounds. I doubt whether I am wise in trying to exhibit smoke-jets to an audience, but I have a special means of projection by which I ought at least to succeed in making them visible. It consists in a device by which the main part of the light from the lamp is stopped at the image of the arc, so that the only light which can reach the screen is light which by diffusion has been diverted out of its course. Thus we shall get an exhibition of a jet of smoke upon the screen, showing bright on a dark ground. The jet issues near the mouth of a resonator of pitch 256. When undisturbed it pursues a straight course, and remains cylindrical. But if a fork of suitable pitch be sounded in the neighbourhood, the jet spreads out into a sort of fan, or even bifurcates, as you see upon the screen. The real motion of the jet cannot of course be ascertained by mere inspection. It consists in a continuously increasing *sinuosity*, leading after a while to complete disruption. If two forks slightly out of unison are sounded together, the jet expands and re-collects itself, synchronously with the audible beats. I should say that my jet is a very coarse imitation of Tyndall's. The nozzle that I am using is much too large. With a proper nozzle, and in a perfectly undisturbed atmosphere—undisturbed not only by sounds, but free from all draughts—the sensitiveness is wonderful. The slightest noise is seen to act instantly and to bring the jet down to a fraction of its former height.

Another important part of Tyndall's work on Sound was carried out as adviser of the Trinity House. When in thick weather the ordinary lights fail, an attempt is made to replace them with sound signals. These are found to vary much in their action, sometimes being heard to a very great distance, and at other times failing to make themselves audible even at a moderate distance. Two explanations have been suggested, depending upon acoustic refraction and acoustic reflection.

Under the influence of variations of temperature refraction occurs in the atmosphere. For example, sound travels more quickly in warm than in cold air. If, as often happens, it is colder above, the upper part of the sound wave tends to lag behind, and the wave is liable to be tilted upwards and so to be carried over the head of the would-be observer on the surface of the ground. This explanation of acoustic refraction by variation of temperature was given by Prof. Osborne Reynolds. As Sir G. Stokes showed, refraction is also caused by wind. The difference between refraction by wind and by temperature variations is that in one case everything turns upon the direction in which the sound is going, while in the second case this consideration is immaterial. The sound is heard by an observer down wind, and not so well by an observer up wind. The explanation by refraction of the frequent failure of sound signals was that adopted by Prof. Henry in America, a distinguished worker upon

this subject. Tyndall's investigations, however, led him to favour another explanation. His view was that sound was actually reflected by atmospheric irregularities. He observed, what appears to be amply sufficient to establish his case, that prolonged signals from fog sirens give rise to echoes audible after the signal has stopped. This echo was heard from the air over the sea, and lasted in many cases a long time, up to 15 seconds. There seems here no alternative but to suppose that reflection must have occurred internally in the atmosphere. In some cases the explanation of the occasional diminished penetration of sound seems to be rather by refraction, and in others by reflection.

Tyndall proved that a single layer of hot air is sufficient to cause reflection, and I propose to repeat his experiment. The source of sound, a toy reed, is placed at one end of one metallic tube, and a sensitive flame at one end of a second. The opposite ends of these tubes are placed near each other, but in a position which does not permit the sound waves issuing from the one to enter the other directly. Accordingly the flame shows no response. If, however, a pane of glass be held suitably, the waves are reflected back and the flame is excited. Tyndall's experiment consists in the demonstration that a flat gas flame is competent to act the part of a reflector. When I hold the gas flame in the proper position, the percipient flame flares; when the flat flame is removed or held at an unsuitable angle, there is almost complete recovery.

It is true that in the atmosphere no such violent transitions of density can occur as are met with in a flame; but, on the other hand, the interruptions may be very numerous, as is indeed rendered probable by the phenomena of stellar scintillation.

The third portion of my subject must be treated very briefly. The guiding idea of much of Tyndall's work on atmospheric particles was the application of an intense illumination to render them evident. Fine particles of mastic, precipitated on admixture of varnish with a large quantity of water, had already been examined by Brucke. Chemically precipitated sulphur is convenient, and allows the influence of size to be watched as the particles grow. But the most interesting observations of Tyndall relate to precipitates in gases caused by the chemical action of the light itself. This may be illustrated by causing the concentrated rays of the electric lamp to pass through a flask containing vapour of peroxide of chlorine. Within a few seconds dense clouds are produced.

When the particles are very small in comparison with the wave length, the laws governing the dispersion of the light are simple. Tyndall pursued the investigation to the case where the particles have grown beyond the limit above indicated, and found that the polarisation of the dispersed light was effected in a peculiar and interesting manner.

Atmospheric dust, especially in London, is largely organic. If,



following Tyndall, we hold a spirit lamp under the track of the light from the electric lamp, the dark spaces, resulting from the combustion of the dust, have all the appearance of smoke.

In confined and undisturbed spaces the dust settles out. I have here a large flask which has been closed for some days. If I hold it to the lamp, the track of the light, plainly visible before entering and after leaving the flask, is there interrupted. This, it will be evident, is a matter of considerable importance in connection with organic germs.

The question of the spontaneous generation of life occupied Tyndall for several years. He brought to bear upon it untiring perseverance and refined experimental skill, and his results are those now generally accepted. Guarding himself from too absolute statements as to other times and other conditions, he concluded that under the circumstances of our experiments life is always founded upon life. The putrefaction of vegetable and animal infusions, even when initially sterilised, is to be attributed to the intrusion of organic germs from the atmosphere.

The universal presence of such germs is often regarded as a hypothesis difficult of acceptance. It may be illustrated by an experiment from the inorganic world. I have here, and can project upon the screen, glass pots, each containing a shallow layer of a supersaturated solution of sulphate of soda. Protected by glass covers, they have stood without crystallising for forty-eight hours. But if I remove the cover, a few seconds or minutes will see the crystallisation commence. It has begun, and long needles are invading the field of view. Here it must be understood that, with a few exceptions, the crystalline germ required to start the action must be of the same nature as the dissolved salt; and the conclusion is that small crystals of sulphate of soda are universally present in the atmosphere.

I have now completed my task. With more or less success I have laid before you the substance of some of Tyndall's contributions to knowledge. What I could not hope to recall was the brilliant and often poetic exposition by which his vivid imagination illumined the dry facts of science. Some reminiscence of this may still be recovered by the reader of his treatises and memoirs; but much survives only as an influence exerted upon the minds of his contemporaries, and manifested in subsequent advances due to his inspiration.

[RAYLEIGH.]

Friday, April 13, 1894.

SIR FREDERICK BRAMWELL, Bart. D.C.L. LL.D. F.R.S. Honorary  
Secretary and Vice-President, in the Chair.

PROFESSOR J. J. THOMSON, M.A. Sc.D. F.R.S.

*Electric Discharge through Gases.*

ONE of the most important and interesting branches of physical science is that which deals with the connection between electrical and chemical effects.

The investigations on electrolysis made within these walls by Davy and Faraday proved that the important class of electrical phenomena associated with the passage of electricity through liquids, are connected in the closest way with chemical action. They proved that no electricity will pass through most liquids unless chemical action occurs, and that for each unit of electricity which passes through the liquid there is a definite amount of chemical decomposition.

This case, though it is one where the laws are most accurately known, is but one among many electrical phenomena which are inseparable from chemical action.

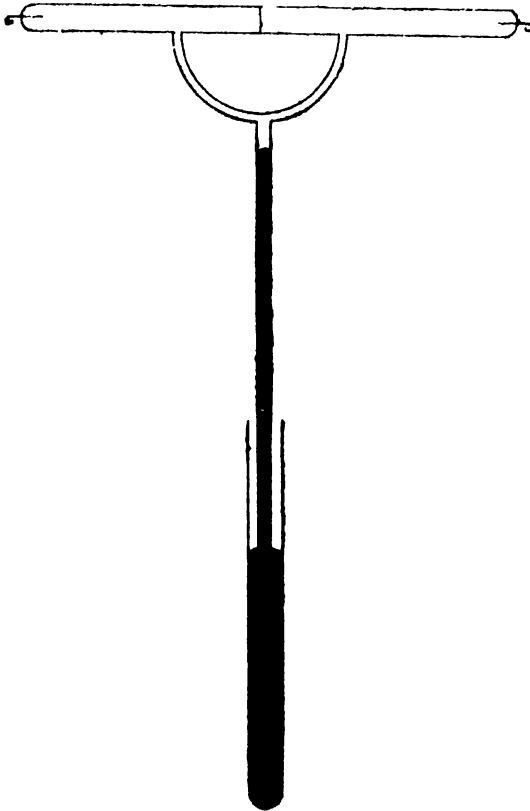
So many instances of this kind have been discovered that we may perhaps venture to hope that we are not far from the time when it will be universally recognised that many of the most fundamental questions in chemistry and electricity are but different aspects of one and the same phenomenon.

Anything which throws light on the connection between electricity and matter, interesting as it is on its own account, acquires additional interest when regarded as elucidating the connection between chemical and electrical effects, and no phenomena seem more suitable for this purpose than those which are the subject of the discourse this evening—the discharge of electricity through gases. For in gases we have matter in the state in which its properties have been most carefully studied, while the investigation of the electrical effects is facilitated by the visibility of the discharge, affording us ocular, and not merely circumstantial, evidence of what is taking place.

The points to which I wish to refer particularly this evening are, firstly, some phenomena connected with the passage of electricity from the gas to the electrode, or from the electrode to the gas; and secondly, some of the properties of the discharge when its course lies entirely in the gas.

By taking a long discharge tube, say, one 50 feet long, and observing the luminous discharge through a rotating mirror, we can trace the course of the luminosity due to a single discharge, say, one due to once breaking the primary circuit of an induction coil; if we do so we find that the luminosity follows the direction of the positive current through the tube. That is, the luminosity begins at the positive electrode, it then rushes down the tube with enormous

Fig 1.



velocity, but when it gets to the negative electrode, it receives a check; it does not disappear at once in that electrode like a rabbit going down a hole, but lingers around the electrode some time before entering it. In consequence of this delay in the positive discharge in getting out of the gas, there is an accumulation of positive electricity in the neighbourhood of the negative electrode until the potential fall at this electrode increases to about 200 or 300 volts.

The positive electricity which accompanies the discharge thus finds considerable difficulty in getting from the gas to the metal, though, as I hope to show you later on, as long as it keeps in the gas, it meets with what we may, in consideration of the views

sometimes enunciated on this subject, call a ridiculously small amount of resistance, its real difficulty is to get out of the gas.

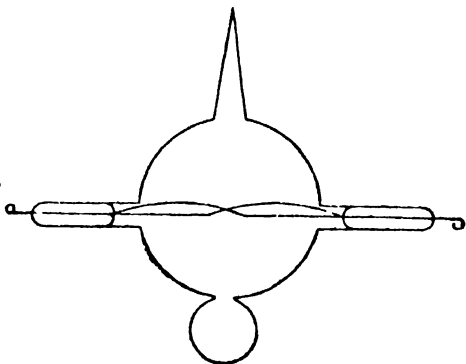
Though this effect has long been known, it is so important that I will venture to show one or two experiments which illustrate it. The arrangement of the first experiment is shown on the screen, the apparatus consists of a main discharge tube, across which is fastened a diaphragm made of excessively thin platinum leaf; there is a side passage from the tube, leading from one side of the diaphragm to the other, this is connected to a barometer tube, and by raising the

cistern containing the mercury I can stop up the passage by a pellet of mercury. We will first observe the discharge when the side passage is open; you see that the discharge, instead of passing across the thin piece of platinum leaf, takes the very much longer route round the side tube, so as to avoid crossing the metal. We will now raise the mercury cistern and close the side tube by a pellet of mercury; the discharge now has no alternative but to cross the metal at some part of its course, and you see that the main portion of the discharge goes back into the main tube.

In the second experiment the metal diaphragm is replaced by a very thin plate of mica; when the side passage is opened the discharge goes round, but when this is closed by a pellet of mercury the discharge prefers to go across the mica than through the mercury.

A second experiment which shows the same thing is the following. Two long electrodes are fused into a bulb, so that the tip of an electrode is a considerable distance from the place where it passes through the glass. We will now send an alternating discharge through the tube, and you will see, I think, that the discharge, instead of going straight across the short distance between the ends of the electrodes, goes from the tip of one electrode to the place where the other passes through the glass, thus staying as long as possible in the gas before passing into the metal. The appearance of the discharge shows that the positive electrode is at the end of the wire, the negative at the junction of the wire with the glass.

Fig 2



Another interesting example of the difficulty the discharge experiences in passing from gas to metal is the discovery made by Professors Liveing and Dewar, that when the discharge passes through a gas containing a large quantity of metallic dust, the light from the discharge, when examined in the spectroscope, does not show any of the lines of the metal.

The difficulty which the positive electricity finds in passing from the gas to the electrode depends a great deal upon the nature of the gas, as well as upon that of the electrode; it is influenced by the position of the gas and the electrode relatively to one another in the electro-chemical series.

I have lately made a series of experiments on this point in the following way. An alternating discharge from a high tension transformer was made to pass between two electrodes fused into a bulb, which could be filled with the gases under examination. Another

electrode connected to an electrometer, passed into the bulb, and was arranged so that it could be moved about from one part of it to the other. When the electrodes were metal and the bulb was filled with the electro-negative gas oxygen, the electrode received a positive charge in whatever part of the bulb it was situated; if now the bulb was filled with hydrogen at atmospheric pressure, then in the regions remote from the arc the electrode received a positive charge, but in the immediate neighbourhood of the arc itself it received a negative charge. When the pressure was reduced the region in which the charge was negative contracted, and finally at pressures about one-third of an atmosphere, seemed to disappear, and the electrode got a slight positive charge in whatever position it was placed. If now, instead of using metallic electrodes we use well-oxidised copper ones, and repeat the experiment in hydrogen, working at a pressure when there was only positive electricity when the electrodes were bright and polished, we find that with the oxidised electrodes every particle of positive electricity is taken out of the tube, and a negative charge is left. This negative charge remains until the copper oxide is completely reduced; when this occurs the negative charge disappears, and is replaced by positive. Thus, under the same conditions as to the nature of the gas and the pressure, the bright copper electrodes leave a positive charge in the gas, while the oxidised ones leave a negative charge.

The most probable explanation of these results seems to me to be the view that the communication of electricity from gas to the electrode, or from the electrode to gas, is facilitated by the temporary formation of something of the nature of a chemical compound between the gas and the metal. In all such compounds the metal is the electro-positive element, and has the positive charge, the gas being the electro-negative and carrying the negative charge. Now consider the case when the negative charge is on the gas, and the positive charge on the metal; then the gas and metal have got the charges proper to them in any compound they may form, and are thus in a fit state to combine, or, according to this view, allow the negative electricity to pass from the gas to the metal. But now, suppose the gas was positively electrified, the gas and the metal have now opposite charges to those proper to them in a compound, and before the union of gas and metal in this state could result in anything but a most unstable compound, an additional process must be gone through—i. e. the charges on the gas and metal must be interchanged. Thus the conditions for the combination of the gas and metal are more complex when the gas is positively electrified than when it is negatively electrified, and thus, on the view that the communication of electricity between the gas and the metal involves a sort of chemical combination, we see that the negative electricity will escape more easily from the gas to the metal than the positive. Now consider the case when the gas was hydrogen, the electrodes oxidised copper; the hydrogen combines now not with the metal, but

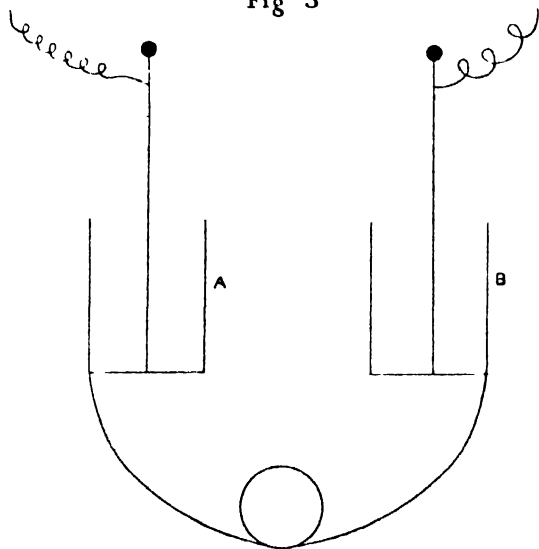
with the oxygen, forming water, in which hydrogen is the electro-positive element; thus, in this case, it is the positively charged hydrogen which is in the state best fitted for pairing. The consequence is, the positive charge would be most readily removed from the gas and the negative left—exactly the opposite to that which occurred when the electrodes were bright. This reversal, as I stated before, is verified by experiment.

I have hitherto only spoken of the phenomena which accompany the passage of electricity from the electrode to the gas, or from the gas to the electrode.

I shall now pass on to consider the properties of the discharge when it is entirely confined to the gas.

We may produce a discharge which, during the whole of its course, shall be confined to the gas in the way represented in the diagram.

Fig 3



The two poles of a Wimshurst machine are connected to the insides of two jars A and B, while the outsides of these jars are connected together by a metal wire wound so as to form a coil. The electricity from the Wimshurst machine charges up the jars, the difference of potential between the poles increases until a spark passes. The passage of the spark puts the insides of the two jars in connection, and the jars are discharged. The discharge of the jar, as was proved from the theory of electro-magnet action by Lord Kelvin more than forty years ago, and shortly afterwards confirmed by the experiments of Feddersen, is an oscillatory one, producing currents surging backwards and forwards through the wires with extraordinary rapidity. The subject of these oscillatory currents is one which in this year is tinged with melancholy. The year had hardly commenced when we lost Hertz, whose splendid work on these electrical oscillations is known to you all. The Managers of this Institution have marked their sense of the importance of this work by devoting a special lecture this session to this work alone, and they have entrusted that lecture to a most distinguished worker in the same field as Hertz. It would therefore be presumptuous on my part to refer in any detail to Hertz's work, but no physicist, and least of all

one who is a member of Maxwell's University, could pass over in silence the death of Hertz.

When Hertz began his magnificent experiments on electric oscillations, there were many theories of electrical action. When he had finished them there was only one, Clerk Maxwell's.

Hertz's work was done with very much quicker vibrations than those produced by the apparatus now on the screen; this, however, gives rise to currents through the coil changing their direction some million times a second. If we place in the coil an exhausted bulb the bulb in reality will be the secondary of an induction coil, and will be exposed to electromotive forces tending to produce circular currents parallel to the plane of the coil.

I will now place a bulb inside this coil, and you see that a circular ring discharge passes through it, and this discharge passes entirely in the gas.

The gas in the bulb now in the coil is the vapour of silicon tetrachloride; it happens to be the bulb which gives a brighter ring than any others I possess.

If this ring discharge passes through air at different pressures, the colour of the discharge changes very considerably. The first bulb I put in was at fairly high pressure, about  $\frac{1}{10}$  of a millimetre or so. I will now put in another at a lower pressure, and then one at a still lower pressure. Mr. Newall, who has been working at the spectra of these discharges, finds that at the pressure in the first bulb the spectrum is due to nitrogen; at the second stage it is due to mercury vapour; the bulb was pumped by a mercury pump, so that there is in the bulb a certain quantity of mercury vapour.

The apple-green colour in the more highly exhausted bulb is due to some compound of sulphur, which has got into the bulb from the sulphuric acid used to dry the gas. Mr. Newall finds that if the ordinary discharge from a coil between electrodes is taken in such a bulb, there is no trace of this sulphur spectrum. He has also found that when the bulb is at a pressure intermediate between what I may call the mercury and the sulphur stage, when the mercury and sulphur lines are both visible, these sets of lines come from different layers, the sulphur lines coming from a layer nearer the surface than the other.

If we take the discharge through a bulb containing oxygen you see that the ring discharge is succeeded by a bright glow; at first the colour is somewhat opaque, but gradually gets more transparent and changes colour. This gives a continuous spectrum crossed by a few bright lines. If we take the discharge through cyanogen you see that the glow is even more persistent than the oxygen, though it is not so bright; all the gases which show this glow belong to the class of substances which polymerise—that is, whose molecules can combine with each other. I imagine that what takes place in bulbs filled with these substances is that the discharge produces a polymeric modification, and that this gradually returns to its original state,

and while doing so gives out a phosphorescent light. It is in accordance with this that at a high temperature where ozone cannot exist a discharge through an oxygen bulb does not show any glow.

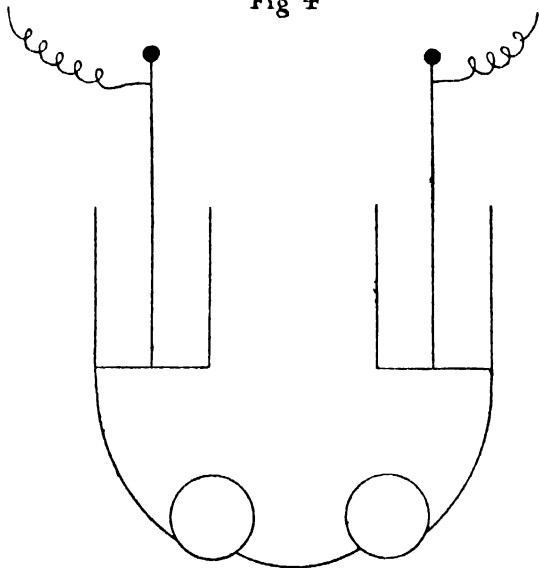
I said at the beginning of this discourse, that gases were exceedingly good conductors of electricity. I will now endeavour to show an experiment which proves that statement. The apparatus which I shall use for this purpose is a slight modification of the one I have used for producing the ring discharge; the only difference is that in the wire connecting the two coatings of the jars there are two loops instead of one. In one of these loops an exhausted bulb is placed to serve as a kind of galvanometer; the brightness of the discharge is an indication of the strength of the current flowing round the coil. If I place a second conductor in the other loop, currents will be started in it and part of the energy of the discharge will be absorbed; this will leave less energy available for the bulb in the first, so that the discharge in this bulb will be dimmer. The effect produced on the discharge will depend upon the conductivity of the substance placed in the second loop.

The effect is not directly proportional to the conductivity, in fact, a perfect conductor would not produce any diminution, nor would an absolute non-conductor; for a given period and with apparatus of given dimensions, there is a certain conductivity which gives a maximum effect; this follows easily from the theory of induction of currents, but at this late period in the evening I will take a shorter course and prove it by an experiment.

I put a piece of brass in this loop, and you see it produces but a small effect upon the brightness of the discharge. Instead of brass, I now insert a plumbago crucible, which, though a conductor, is not nearly so good a one as the brass, and you see the discharge in the indicating bulb is completely stopped.

I will now place in the second loop an exhausted bulb; you see it produces a decided diminution in the intensity of the discharge in the galvanometer bulb. I now replace the bulb by another of the same size containing dilute sulphuric acid; you see it does not

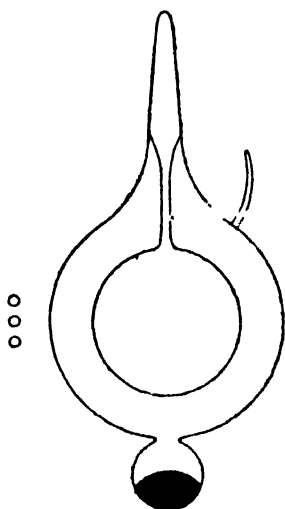
Fig 4





produce nearly so large an effect as the exhausted bulb: this might be due, as we have seen, to the sulphuric acid being either too good or too bad a conductor. I can show that it is the latter by putting a bulb in filled with a stronger solution, which has a higher conductivity than the weak solution; if the smallness of the effect produced by the weak acid were due to its being a better conductor than the gas, then increasing the conductivity would still further diminish the effect of the acid, you see, on the contrary, that the strong acid produces a distinctly greater effect than the weak, hence the rarefied gas in the bulb is a better conductor even than the strong electrolyte. Let us consider for a moment the molecular conductivities of the two substances, the rarefied gas and the electrolyte. The pressure of the gas is about  $\frac{1}{100}$  of a millimetre, while in the electrolyte there are sufficient

Fig 5.



molecules of the acid to produce, if they were in the gaseous state, a pressure of more than 100 atmospheres; thus the conductivity of the gas estimated per molecule is about 10 million times that of the acid, this is greater than the molecular conductivity of even the best conducting metals.

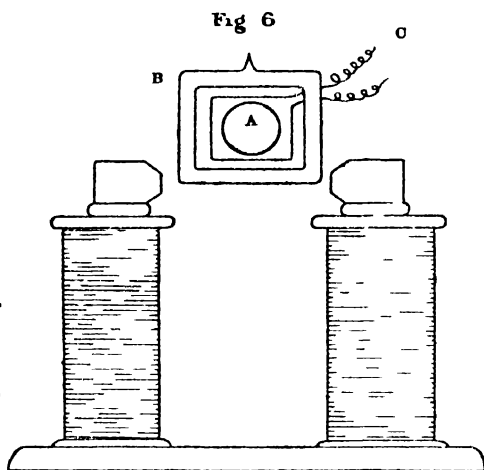
If the pressure of the gas is diminished below a certain point, the conductivity begins to diminish. I have here an experiment which I hope will show this. The apparatus (Fig. 5) consists of two bulbs, one outside the other; the inner bulb contains air at a low pressure, while the space between the two bulbs is a very high vacuum containing practically nothing but a little mercury and its vapour. The amount of mercury vapour in this space is, at the temperature of the room, exceedingly small, but as the apparatus is heated the vapour pressure increases, and we are thus able to produce a fairly wide range of pressure in the space between the bulbs. The outer sphere is surrounded by the coil connecting the outer coatings of the two Leyden jars. When the space between the bulbs is a conductor, the alternating currents circulating in the coil will induce in this conductor currents whose inductive effect is opposite to that of the currents in the coil; and in this case this layer will screen off from the inner bulb the electromotive force due to the alternating currents in the coil. If, on the other hand, the space between the bulbs is a non-conductor, the inner bulb will be exposed to the full effect of these forces. We now try the experiment: you observe that when the mercury is cold, and consequently the pressure in the space between the bulbs very low, a bright discharge passes through the inner bulb, while the space between the bulbs remains quite dark; when we heat

the mercury so as to increase the pressure of its vapour, a bright discharge passes through the outer layer, while the inner bulb is quite dark; the outer layer is now a conductor, and by its action screens off from the inner bulb the induction of the coil.

The last experiment I have to show is one on the effect produced by a magnetic field on the discharge.

When the discharge has to flow across the lines of magnetic force, the pressure of the magnetic field retards the discharge; when however, the discharge flows along the lines of magnetic force, the discharge is helped by the magnetic field. This is shown in the following experiment. A is a bulb; B a square tube, one side of which is placed between the poles of an electromagnet; the coil C, which connects the outside coatings of the jars, can be adjusted so that when the magnet is "off," the discharge passes through

the bulb but not round the square tube; when, however, the magnet is "on," the discharge passes in the square tube but not in the bulb. In the square tube the discharge passes along the lines of magnetic force and is helped; in the bulb it passes across them and is retarded.



[J. J. T.]

Friday, May 18, 1894.

SIR FREDERICK BRAMWELL, Bart. D.C.L. LL.D. F.R.S. Honorary  
Secretary and Vice-President, in the Chair.

PROFESSOR A. M. WORTHINGTON, M.A. F.R.S.

*The Splash of a Drop and Allied Phenomena.*

THE splash of a drop is a transaction which is accomplished in the twinkling of an eye, and it may seem to some that a man who proposes to discourse on the matter for an hour must have lost all sense of proportion. If that opinion exists, I hope this evening to be able to remove it and to convince you that we have to deal with an exquisitely regulated phenomenon, and one which very happily illustrates some of the fundamental properties of fluids. It may be mentioned also that the recent researches of Lenard in Germany and J. J. Thomson at Cambridge, on the curious development of electrical charges that accompanies certain kinds of splashes, have invested with a new interest any examination of the mechanics of the phenomenon. It is to the mechanical and not to the electrical side of the question that I shall call your attention this evening.

The first well directed and deliberate observations on the subject that I am acquainted with were made by a schoolboy at Rugby some twenty years ago, and were reported by him to the Rugby Natural History Society. He had observed that the marks of accidental splashes of ink-drops that had fallen on some smoked glasses with which he was experimenting, presented an appearance not easy to account for. Drops of the same size falling from the same height had made always the same kind of mark, which when carefully examined with a lens showed that the smoke had been swept away in a system of minute concentric rings and fine striæ. Specimens of such patterns, obtained by letting drops of mercury, alcohol and water fall on to smoked glass, are thrown on the screen, and the main characteristics are easily recognised. Such a pattern corresponds to the footprints of the dance that has been performed on the surface, and though the drop may be lying unbroken on the plate, it has evidently been taking violent exercise, and were our vision acute enough we might observe that it was still palpitating after its exertions.

A careful examination of a large number of such footprints showed that any opinion that could be formed therefrom of the nature of the motion of the drop must be largely conjectural, and it occurred to me about eighteen years ago to endeavour by means of the illumi-

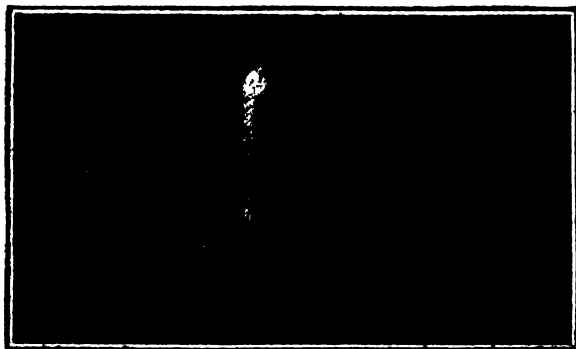
INSTANTANEOUS PHOTOGRAPHS  
OF THE SPLASH OF A WATER-DROP FALLING INTO MILK.



Time after contact = .0262 sec.



Time after contact = .0391 sec.



nation of a suitably timed electric spark to watch a drop through its various changes on impact.

The reason that with ordinary continuous light nothing can be satisfactorily seen of the splash, is not that the phenomenon is of such short duration, but because the changes are so rapid that before the image of one stage has faded from the eye the image of a later and quite different stage is superposed upon it. Thus the resulting impression is a confused assemblage of all the stages, as in the photograph of a person who has not sat still while the camera was looking at him. The problem to be solved experimentally was therefore this: to let a drop of definite size fall from a definite height in comparative darkness on to a surface, and to illuminate it by a flash of exceedingly short duration at any desired stage, so as to exclude all the stages previous and subsequent to the one thus picked out. The flash must be bright enough for the image of what is seen to remain long enough on the eye for the observer to be able to attend to it, even to shift his attention from one part to another, and thus to make a drawing of what is seen. If necessary the experiment must be capable of repetition, with an exactly similar drop falling from exactly the same height, and illuminated at exactly the same stage. Then, when this stage has been sufficiently studied, we must be able to arrange with another similar drop to illuminate it at a rather later stage, say  $\frac{1}{1000}$  second later, and in this way to follow step by step the course of the whole phenomenon.

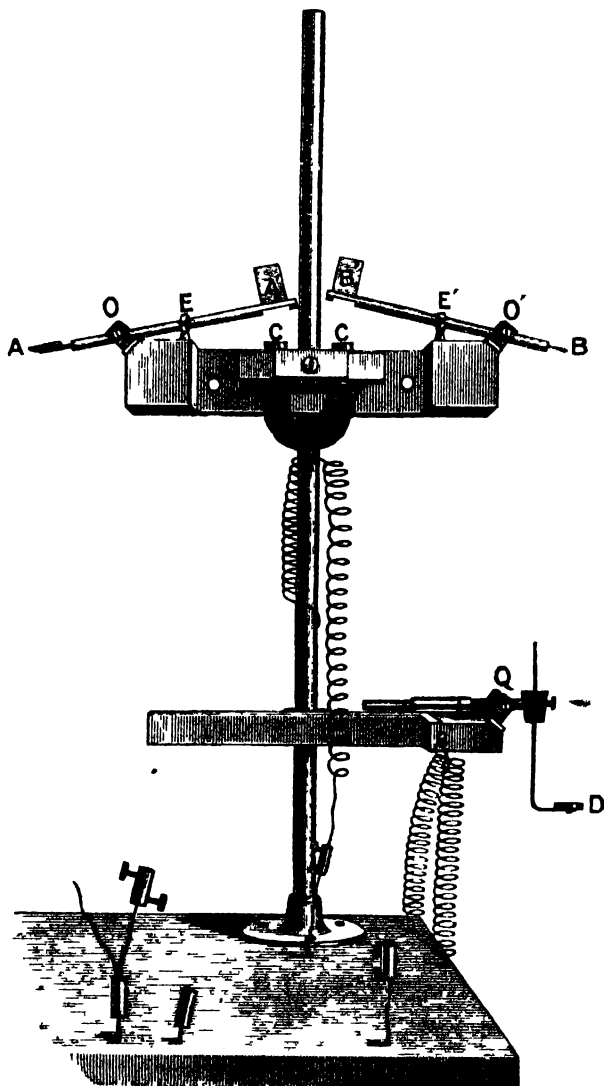
The apparatus by which this has been accomplished is on the table before you. Time will not suffice to explain how it grew out of earlier arrangements very different in appearance, but its action is very simple and easy to follow by reference to the diagram (Fig. 1).

A A' is a light wooden rod rather longer and thicker than an ordinary lead pencil, and pivoted on a horizontal axle O. The rod bears at the end A a small deep watch-glass, or segment of a watch-glass, whose surface has been smoked, so that a drop even of water will lie on it without adhesion. The end A' carries a small strip of tinned iron, which can be pressed against and held down by an electromagnet C C'. When the current of the electromagnet is cut off the iron is released, and the end A' of the rod is tossed up by the action of a piece of india-rubber stretched catapult-wise across two pegs at E, and by this means the drop resting on the watch-glass is left in mid-air free to fall from rest.

B B' is a precisely similar rod worked in just the same way, but carrying at B a small horizontal metal ring, on which an ivory timing sphere of the size of a child's marble can be supported. On cutting off the current of the electromagnet the ends A' and B' of the two levers are simultaneously tossed up by the catapults, and thus drop and sphere begin to fall at the same moment. Before, however, the drop reaches the surface on which it is to impinge, the timing sphere strikes a plate D attached to one end of a third lever pivoted at Q, and thus breaks the contact between a platinum wire bound to the

under side of this lever and another wire crossing the first at right angles. This action breaks an electric current which has traversed a second electromagnet F' (Fig. 2), and releases the iron armature N

FIG 1

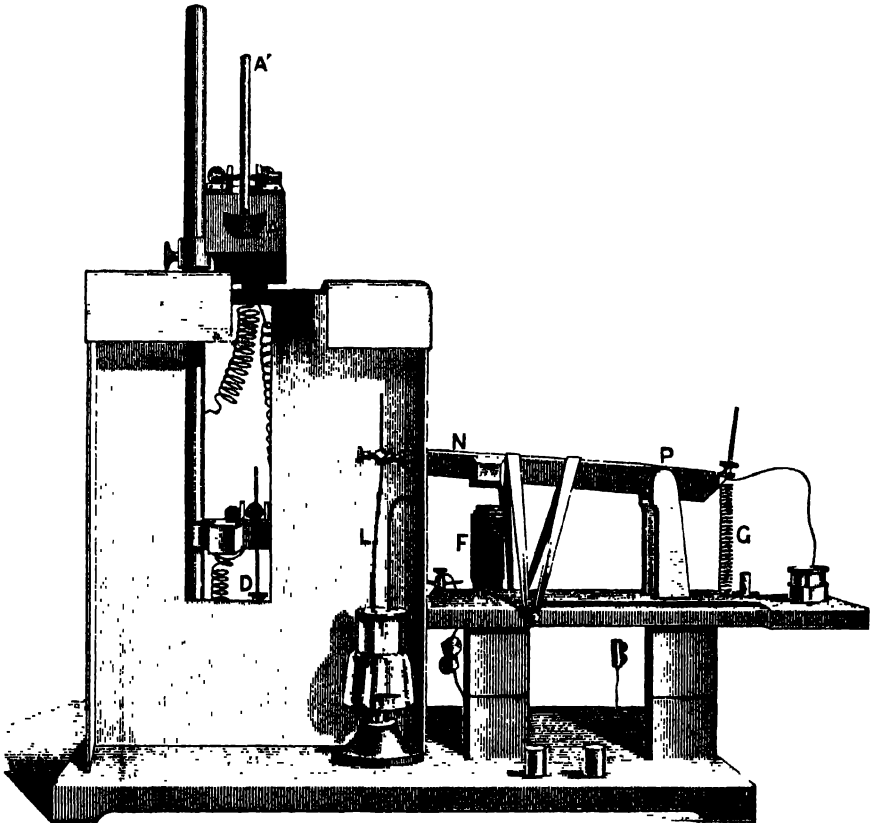


of the lever N P, pivoted at P, thus enabling a strong spiral spring G to lift a stout brass wire L out of mercury, and to break at the surface of the mercury a strong current that has circulated round the primary circuit of a Ruhmkorff's induction coil ; this produces at the surface

of the mercury a bright self-induction spark in the neighbourhood of the splash, and it is by this flash that the splash is viewed. The illumination is greatly helped by surrounding the place where the splash and flash are produced by a white cardboard enclosuro, seen in Fig. 2, from whose walls the light is diffused.

It will be observed that the time at which the spark is made will depend on the distance that the sphere has to fall before striking the plate D, for the subsequent action of demagnetising F and pulling

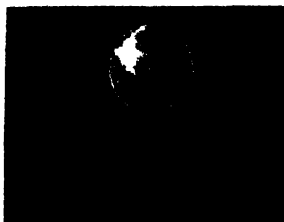
FIG. 2.



the wire L out of the mercury in the cup H is the same on each occasion. The *modus operandi* is consequently as follows:—The observer, sitting in comparative but by no means complete darkness, faces the apparatus as it appears in Fig. 2, presses down the ends A' B' of the levers first described, so that they are held by the electromagnet C (Fig. 1). Then he presses the lever NP down on the electromagnet F, sets the timing sphere and drop in place, and then by means of a bridge between two mercury cups, short-circuits and

FIRST SERIES.

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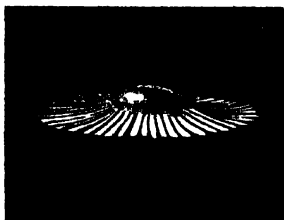
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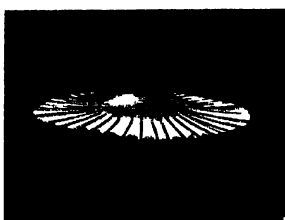
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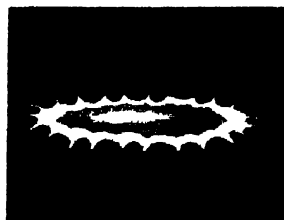
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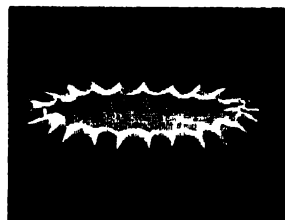
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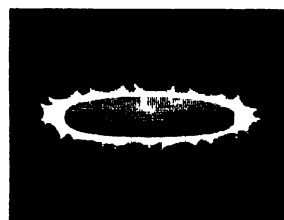
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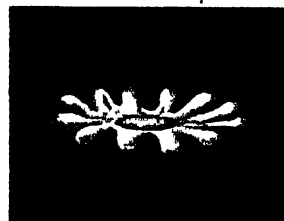
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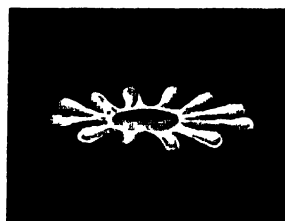
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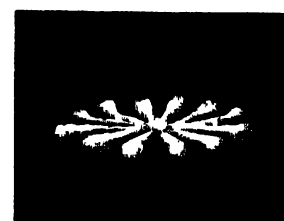
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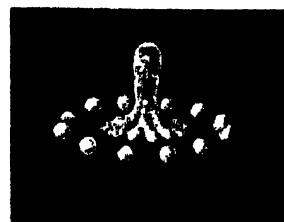
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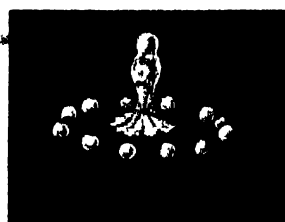
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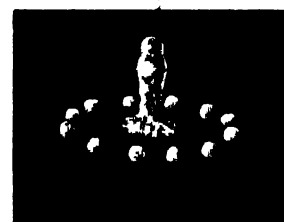
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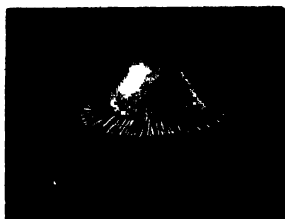
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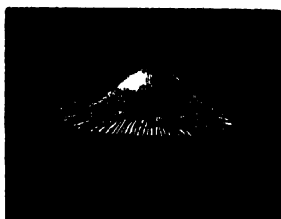


# FIRST SERIES.

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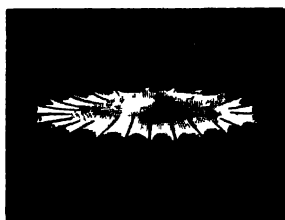
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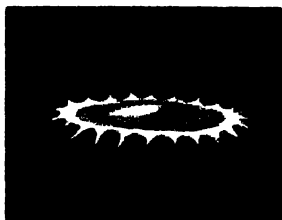
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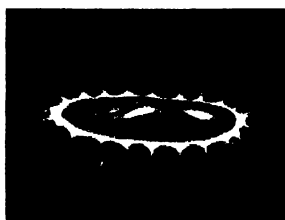
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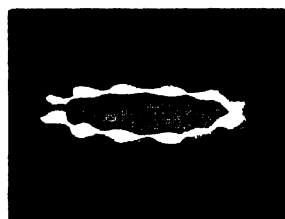
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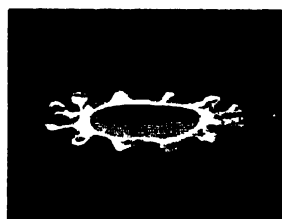
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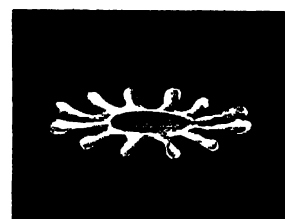
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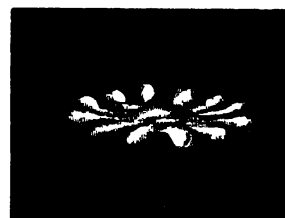
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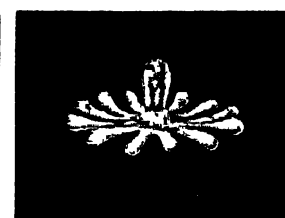
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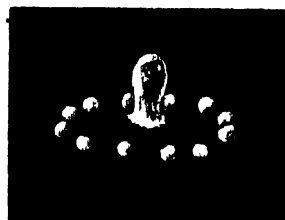
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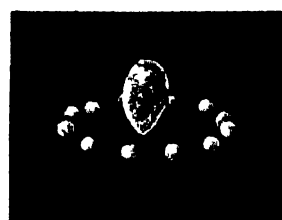
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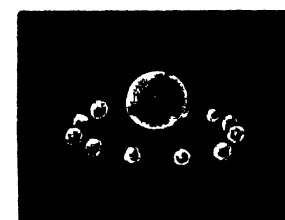
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thus cuts off the current of the electromagnet C. This lets off drop and sphere, and produces the flash. The stage of the phenomenon that is thus revealed having been sufficiently studied by repetition of the experiment as often as may be necessary, he lowers the plate D a fraction of an inch and thus obtains a later stage. Not only is any desired stage of the phenomenon thus easily brought under examination, but the apparatus also affords the means of measuring the time interval between any two stages. All that is necessary is to know the distance that the timing sphere falls in the two cases. Elementary dynamics then give us the interval required. Thus, if the sphere falls one foot and we then lower D  $\frac{1}{4}$  inch, the interval between the corresponding stages will be about  $\cdot 0026$  second.

Having thus described the apparatus, which I hope shortly to show you in action, I pass to the information that has been obtained by it.

This is contained in a long series of drawings, of which a selection will be presented on the screen. The First Series that I have to show represents the splash of a drop of mercury  $0\cdot 15$  inch in diameter that has fallen 3 inches on to a smooth glass plate. It will be noticed that very soon after the first moment of impact, minute rays are shot out in all directions on the surface. These are afterwards overflowed or united, until, as in Fig. 8, the outline is only slightly rippled. Then (Fig. 9) main rays shoot out, from the ends of which in some cases minute droplets of liquid would split off, to be left lying in a circle on the plate, and visible in all subsequent stages. By counting these droplets when they were thus left, the number of rays was ascertained to have been generally about 24. This exquisite shell-like configuration shown in Fig. 9, marks about the maximum spread of the liquid, which, subsiding in the middle, afterwards flows into an annulus or rim with a very thin central film, so thin, in fact, as often to tear more or less irregularly. This annular rim then divides or segments (Figs. 14, 15, 16) in such a manner as to join up the rays in pairs, and thus passes into the 12-lobed annulus of Fig. 16. Then the whole contracts, but contracts most rapidly between the lobes, the liquid then being driven into and feeding the arms, which follow more slowly. In Fig. 21 the end of this stage is reached, and now the arms continuing to come in, the liquid rises in the centre; this is, in fact, the beginning of the rebound of the drop from the plate. In the case before us the drops at the ends of the arms now break off (Fig. 25), while the central mass rises in a column which just fails itself to break up into drops, and falls back into the middle of the circle of satellites which, it will be understood, may in some cases again be surrounded by a second circle of the still smaller and more numerous droplets that split off the ends of the rays in Fig. 9. The whole of the 30 stages described are accomplished in about  $\frac{1}{20}$  second, so that the average interval between them is about  $\frac{1}{600}$  second.

It should be mentioned that it is only in rare cases that the subordinate drops, seen in the last six figures, are found lying in a very

complete circle after all is over, for there is generally some slight disturbing lateral velocity which causes many to mingle again with the central drop, or with each other. But even if only half or a quarter of the circle is left, it is easy to estimate how many drops, and therefore how many arms there have been. It may be mentioned that sometimes the surface of the central lake of liquid, Figs. 14, 15, 16, 17, was seen to be covered with beautiful concentric ripples, not shown in the figures.

The question now naturally presents itself, why should the drop behave in this manner? In seeking the answer it will be useful to ask ourselves another question. What should we have expected the drop to do? Well, to this I suppose most people would be inclined, arguing from analogy with a solid, to reply that it would be reasonable to expect the drop to flatten itself, and even very considerably flatten itself, and then, collecting itself together again, to rebound, perhaps as a column such as we have seen, but not to form this regular system of rays and arms and subordinate drops.

Now this argument from analogy with a solid is rather misleading, for the forces that operate in the case of a solid sphere that flattens itself and rebounds, are due to the bodily elasticity which enables it not only to resist, but also to recover from any distortion of shape or altering of its internal parts past each other. But a liquid has no power of recovering from such internal shear, and the only force that checks the spread, and ultimately causes the recovery of shape is the *surface tension*, which arises from the fact that the surface layers are always in a state of extension and always endeavouring to contract. Thus we are at liberty when dealing with the motions of the drop to think of the interior liquid as not coherent, provided we furnish it with a suitable elastic skin. Where the surface skin is sharply curved outwards, as it is at the sharp edge of the flattened disc, there the interior liquid will be strongly pressed back. In fact the process of flattening and recoil is one in which energy of motion is first expended in creating fresh liquid surface, and subsequently recovered as the surface contracts. The transformation is, however, at all moments accompanied by a great loss of energy as heat. Moreover, it must be remembered that the energy expended in creating the surface of the satellite drops is not restored if these remain permanently separate. Thus the surface tension explains the recoil, and it is also closely connected with the formation of the subordinate rays and arms. To explain this it is only necessary to remind you that a liquid cylinder is an unstable configuration. As you know, any fine jet becomes beaded and breaks into drops, but it is not necessary that there should be any flow of liquid along the jet; if, for example, we could realise a rod of liquid of the shape and size of this ruler and liberate it in the air, it would not retain its cylindrical shape, but would segment or divide itself up into a row of drops regularly disposed according to a definite and very simple numerical law, viz. that the distances between the

centres of contiguous drops would be equal to the circumference of the cylinder. This can be shown by calculation to be a consequence of the surface tension, and the calculation has been closely verified by experiment. If the liquid cylinder were liberated on a plate, it would still topple into a regular row of drops, but they would be further apart; this was shown by Plateau. Now imagine the cylinder bent into an annulus. It will still follow the same law,\* i. e. it will topple into drops just as if it were straight. This I can show you by a direct experiment. I have here a small thick disc of iron, with an accurately planed face and a handle at the back. In the face is cut a circular groove, whose cross section is a semicircle. I now lay this disc face downwards on the horizontal face of the lantern condenser, and through one of two small holes bored through to the back of the disc I fill the groove with quicksilver. Now, suddenly lifting the disc from the plate I release an annulus of liquid, which splits into the circle of very equal drops which you see projected on the screen. You will notice that the main drops have between them still smaller ones, which have come from the splitting up of the thin cylindrical necks of liquid which connected the larger drops at the last moment.

Now this tendency to segment or topple into drops, whether of a straight cylinder or of an annulus, is the key to the formation of the arms and satellites, and indeed to much that happens in all the splashes that we shall examine. Thus in Fig. 12 we have an annular rim, which in Figs. 13 and 14 is seen to topple into lobes by which the rays are united in pairs, and even the special rays that are seen in Fig. 9 owe their origin to the segmentation of the rim of the thin disc into which the liquid has spread. The proceeding is probably exactly analogous to what takes place in a sea wave that curls over in calm weather on a slightly sloping shore. Any one may notice how, as it curls over, the wave presents a long smooth edge, from which at a given instant a multitude of jets suddenly shoot out, and at once the back of the wave, hitherto smooth, is seen to be furrowed or "combed." There can be no doubt that the cylindrical edge topples into alternate convexities and concavities; at the former the flow is helped, at the latter hindered, and thus the jets begin, and special lines of flow are determined. In precisely the same way the previously smooth circular edge of Fig. 8 topples, and determines the rays and lines of flow of Fig. 9.

Before going on to other splashes I will now endeavour to reproduce a mercury splash of the kind I have described, in a manner that shall be visible to all. For this purpose I have reduplicated the apparatus which you have seen, and have it here so arranged that I can let the drop fall on to the horizontal condenser plate of the lantern, through which the light passes upwards, to be afterwards thrown upon this

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\* See Worthington on the "Spontaneous Segmentation of a Liquid Annulus," Proc. Roy. Soc. No. 200, p. 49 (1879).

screen. The illuminating flash will be made inside the lantern, where the arc light would ordinarily be placed. I have now set a drop of mercury in readiness and put the timing sphere in place, and now if you will look intently at the middle of the screen I will darken the room and let off the splash. (The experiment was repeated four or five times, and the figures seen were like those of Series X.) Of course all that can be shown in this way is the outline, or rather a horizontal section of the splash; but you are able to recognise some of the configurations already described, and will be the more willing to believe that a momentary view is after all sufficient to give much information if one is on the alert and has acquired skill by practice.

The general features of the splash that we have examined are not merely characteristic of the liquid mercury, but belong to all splashes of a liquid falling on to a surface which it does not wet, provided the height of fall or size of the drop are not so great as to cause complete disruption,\* in which case there is no recovery and rebound. Thus a drop of milk falling on to smoked glass will, if the height of fall and size of drop are properly adjusted, give forms very similar to those presented by a drop of mercury. The whole course of the phenomenon depends, in fact, mainly on four quantities only: (1) the size of the drop; (2) the height of fall; (3) the value of the surface tension; (4) the viscosity of the liquid.

The next series of drawings illustrates the splash of a drop of water falling into water.

In order the better to distinguish the liquid of the original drop from that into which it falls, the latter was coloured with ink or with an aniline dye, and the drop itself was of water rendered turbid with finely-divided matter in suspension. Finally drops of milk were found to be very suitable for the purpose, the substitution of milk for water not producing any observable change in the phenomenon.

In Series II. the drop fell 3 inches, and was  $\frac{1}{2}$  inch in diameter.

[In most of the figures of this and of succeeding series the central white patch represents the original drop, and the white parts round it represent those raised portions of the liquid which catch the light. The numbers at the side of each figure give the time interval in seconds from the occurrence of the first figure, or of the figure marked  $T = 0$ .]

It will be observed that the drop flattens itself out somewhat, and descends at the bottom of a hollow with a raised beaded edge (Fig. 2). This edge would be smooth and circular but for the instability which causes it to topple into drops. As the drop descends the hollow becomes wider and deeper, and finally closes over the

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\* Readers who wish a more detailed account of a greater variety of splashes are referred to papers by the author. Proc. Roy. Soc. vol. xxv. pp. 261 and 498 (1877); and vol. xxxiv. p. 217 (1882).

# SERIES II.

*The Splash of a Drop, followed in detail by Instantaneous Illumination.*

Diameter of Drop,  
 $\frac{1}{8}$  inch.  
Height of Fall,  
 $8\frac{1}{2}$  inches.



Time in  
Seconds.  
 $\tau = 0$



$\tau = 0$



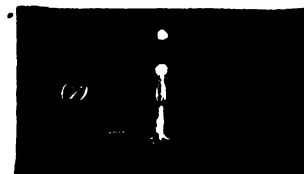
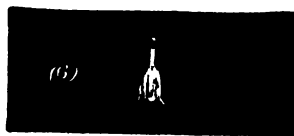
$\tau = .0097$



$\tau = .0392$



$\tau = .0392$



$\tau = .0979$



$\tau = .1095$

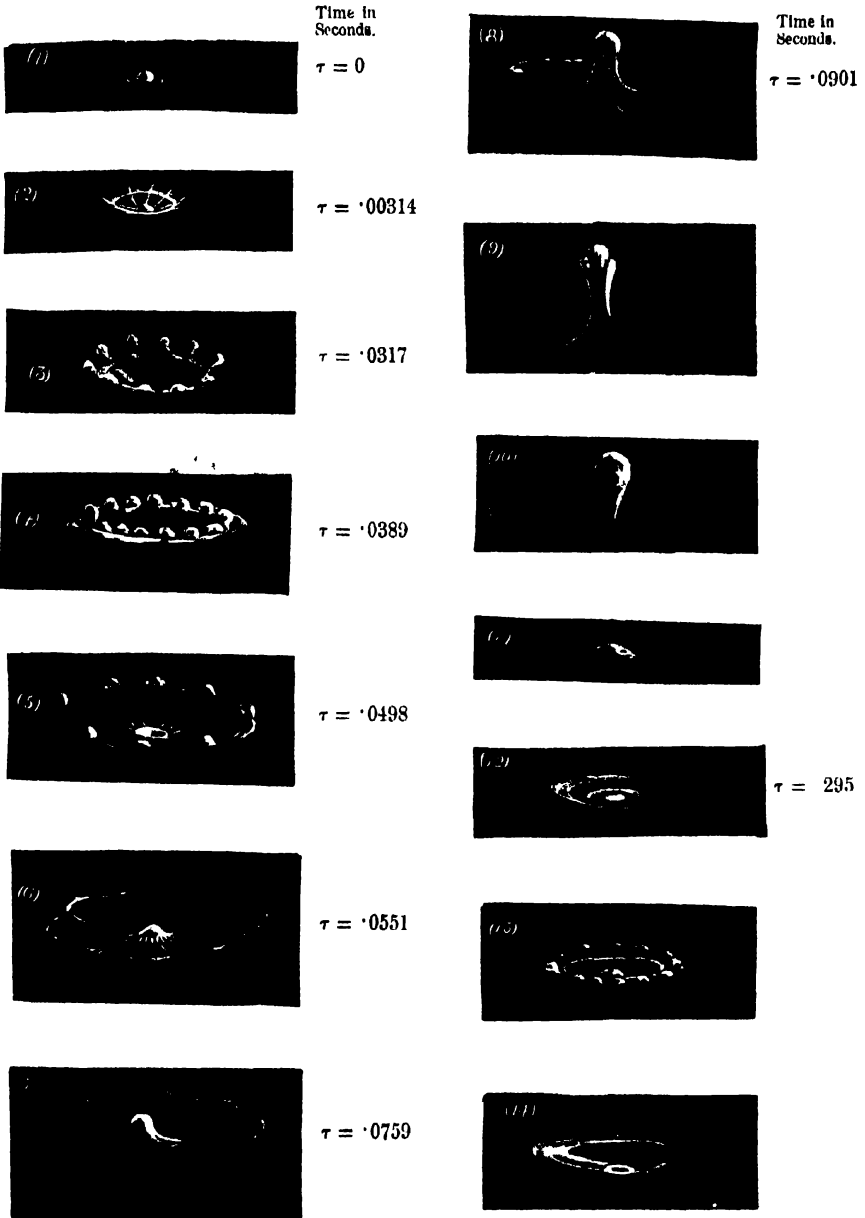


$\tau = .167$

# SERIES III.

## *The Splash of a Drop, followed in detail by Instantaneous Illumination—cont.*

Diameter of Drop,  $\frac{1}{8}$  inch. Height of Fall, 1 ft. 5 in.



drop (Fig. 3), which, however, soon again emerges as the hollow flattens out, appearing first near, but still below the surface (Fig. 4), in a flattened, lobed form, afterwards rising as a column somewhat mixed with adherent water, in which traces of the lobes are at first very visible.

The rising column, which is nearly cylindrical, breaks up into drops before or during its subsequent descent into the liquid. As it disappears below the surface the outward and downward flow causes a hollow to be again formed, up the sides of which an annulus of milk is carried, while the remainder descends to be torn again a second time into a vortex ring, which, however, is liable to disturbance from the falling in of the drops which once formed the upper part of the rebounding column.

It is not difficult to recognise some features of this splash without any apparatus beyond a cup of tea and a spoonful of milk. Any drinker of afternoon tea, after the tea is poured out and before the milk is put in, may let the milk fall into it drop by drop from one or two inches above it. The rebounding column will be seen to consist almost entirely of milk, and to break up into drops in the manner described, while the vortex ring, whose core is of milk, may be seen to shoot down into the liquid. But this is better observed by dropping ink into a tumbler of clear water.

Let us now increase the height of fall to 17 inches. Series III. exhibits the result. All the characteristics of the last splash are more strongly marked. In Fig. 1 we have caught sight of the little raised rim of the hollow before it has beaded, but in Fig. 2 special channels of easiest flow have been already determined. The number of ribs and rays in this basket-shaped hollow seemed to vary a good deal with different drops, as also did the number of arms and lobes seen in later figures, in a somewhat puzzling manner, and I have made no attempt to select drawings which are in agreement in this respect. It will be understood that these rays contain little or none of the liquid of the drop, which remains collected together in the middle. Drops from these rays or from the larger arms and lobes of subsequent figures are often thrown off high into the air. In Figs. 3 and 4 the drop is clean gone below the surface of the hollow, which is now deeper and larger than before. The beautiful beaded annular edge then subsides, and in Fig. 5 we see the drop again, and in Fig. 6 it begins to emerge. But although the drop has fallen from a greater height than in the previous splash, the energy of the impact, instead of being expended in raising the same amount of liquid to a greater height, is now spent in lifting a much thicker adherent column to about the same height as in the last splash. There was sometimes noticed, as is seen in Fig. 9, a tendency in the water to flow up past the milk, which, still comparatively unmixed with water, rides triumphant on the top of the emergent column. The greater relative thickness of this column prevents it splitting into drops, and Figs. 10 and 11 show it descending below the surface to form the hollow of



Fig. 12, up the sides of which an annular film of milk is carried (Figs. 12 and 13), having been detached from the central mass, which descends to be torn again, this time centrally into a well-marked vortex ring.

If we keep to the same size of drop and increase the fall to something over a yard, no great change occurs in the nature of the splash, but the emergent column is rather higher and thinner and shows a tendency to split into drops.

When, however, we double the volume of the drop and raise the height of fall to 52 inches, the splash of Series IV. is obtained, which is beginning to assume quite a different character. The raised rim of the previous series is now developed into a hollow shell of considerable height, which tends to close over the drop. This shell or dome is a characteristic feature of all splashes made by large drops falling from a considerable height, and is extremely beautiful. In the splash at present under consideration it does not always succeed in closing permanently, but opens out as it subsides, and is followed by the emergence of the drop (Fig. 8). In Fig. 9 the return wave overwhelms the drop for an instant, but it is again seen at the summit of the column in Fig. 10.

But on other occasions the shell or dome of Figs. 4 and 5 closes permanently ~~over~~ the imprisoned air, the liquid then flowing down the sides, which become thinner and thinner, till at length we are left with a large bubble floating on the water (see Series V.). It will be observed that the flow of liquid down the sides is chiefly along definite channels, which are probably determined by the arms thrown up at an earlier stage. The bubble is generally creased by the weight of the liquid along these channels. It must be remembered that the base of the bubble is in a state of oscillation, and that the whole is liable to burst at any moment, when such figures as 6 and 7 of the previous series will be seen.

Such is the history of the building of the bubbles which big rain-drops leave on the smooth water of a lake, or pond, or puddle. It is only the bigger drops that can do it, and reference to the number at the side of Fig. 5 of Series IV. shows that the dome is raised in about two-hundredths of a second. Should the domes fail to close, or should they open again, we have the emergent columns which any attentive observer will readily recognise, and which have never been better described than by Mr. R. L. Stevenson, who, in his delightful 'Inland Voyage,' speaks of the surface of the Belgian canals along which he was canoeing, as thrown up by the rain into "an infinity of little crystal fountains."

Very beautiful forms of the same type indeed, but different in detail, are those produced by a drop of water falling into the lighter and more mobile liquid, petroleum.

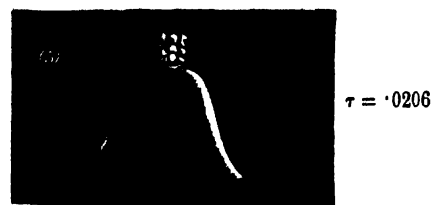
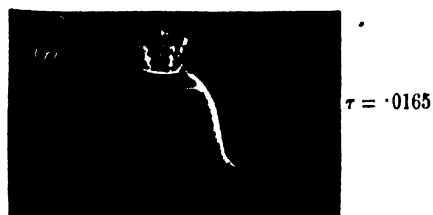
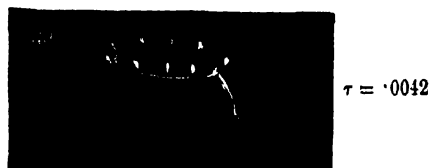
It will now be interesting to turn to the splash that is produced when a solid sphere, such as a child's marble, falls into water.

I found to my great surprise that the character of the splash at

# SERIES IV.

*The Splash of a Drop, followed in detail by Instantaneous Illumination—cont.*

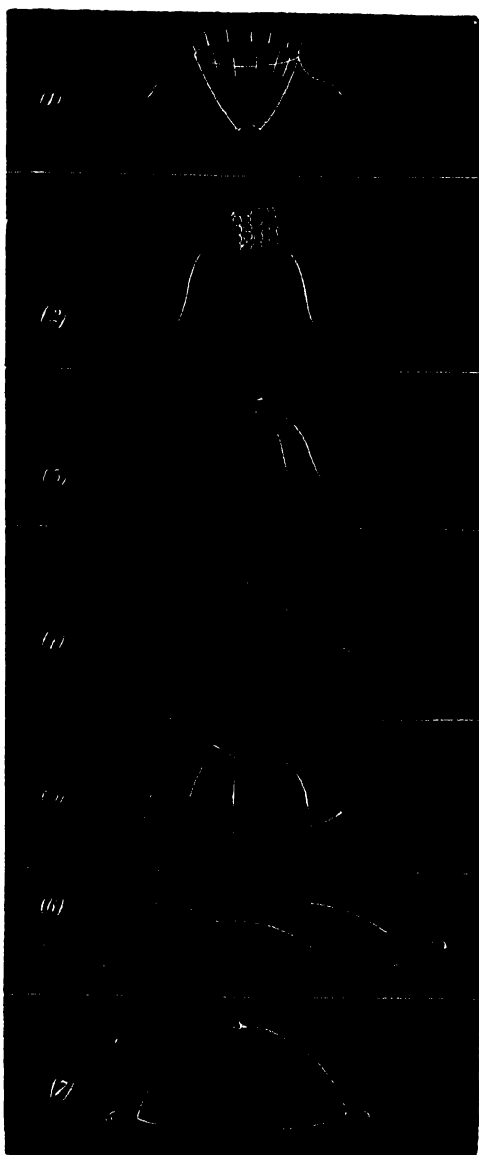
Diameter of Drop,  $\frac{1}{4}$  inch. Height of Fall, 4 ft. 4 in.



## SERIES V.

### *The Splash of a Drop, followed in detail by Instantaneous Illumination— continued.*

The Size of Drop and Height of Fall are the same as before, but the hollow shell (see figs. 4 and 5 of the previous Series) does not succeed in opening, but is left as a bubble on the surface. This explains the formation of bubbles when *big* raindrops fall into a pool of water.

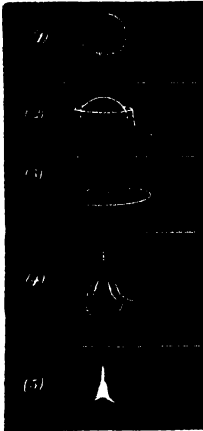


**SERIES VI., VII., VIII.**

*Splash of a Solid Sphere (a marble  $\frac{1}{2}$  inch in diameter falling 2 feet into water).*

**SERIES VI.**

*When the sphere is dry and polished.*



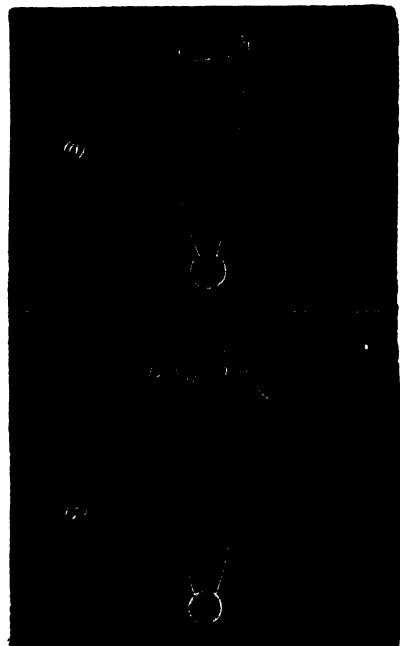
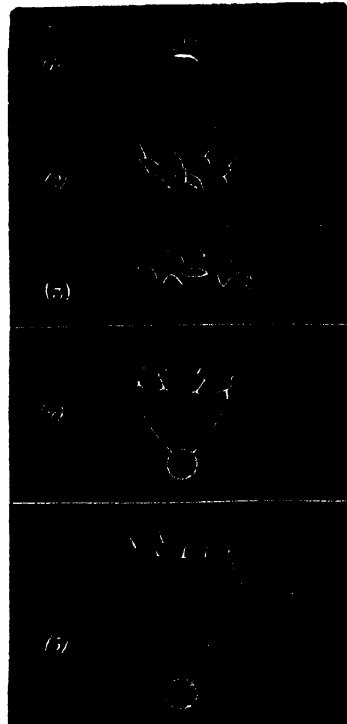
**SERIES VII.**

*When the sphere is not well dried and polished.*



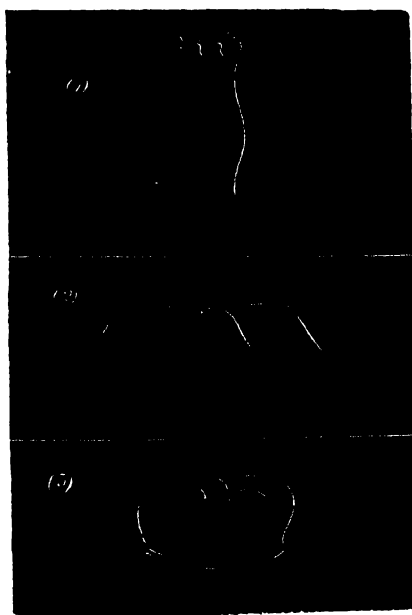
**SERIES VIII.**

*When the sphere is rough or wet.*



*Splash of a Solid Sphere—continued.*

**SERIES IX.**



When the sphere is rough or wet, and falls  
above 5 feet.

any rate up to a height of fall of 4 or 5 feet, depends entirely on the state of the surface of the sphere. A polished sphere of marble about 0.6 of an inch in diameter, rubbed very dry with a cloth just before-hand and dropped from a height of 2 feet into water, gave the figures of Series VI., in which it is seen that the water spreads over the sphere so rapidly, that it is sheathed with the liquid even before it has passed below the general level of the surface. The splash is insignificantly small and of very short duration. If the drying and polishing be not so perfect, the configurations of Series VII. are produced; while if the sphere be roughened with sand-paper, or *left wet*, Series VIII. is obtained, in which it will be perceived that, as was the case with a liquid drop, the water is driven away laterally, forming the ribbed basket-shaped hollow, which, however, is now prolonged to a great depth, the drop being followed by a cone of air, while the water seems to find great difficulty in wetting the surface completely. Part of this column of air was carried down at least 16 inches, and then only detached when the sphere struck the bottom of the vessel.

Figs. 6 and 7 show the crater falling in, but this did not always happen, for the walls often closed over the hollow exactly as in Figs. 4 and 5 of Series IV. Meanwhile the long and nearly cylindrical portion below breaks up into bubbles which rise quickly to the surface.

By increasing the fall to 5 feet we obtain the figures of Series IX. The tube of Fig. 1 corresponds to the dome of Series IV. and V., and is not only elevated to a surprising height, but is also in the act of cleaving (the outline being approximately that of the unduloid of M. Plateau). Figs. 2 and 3 show the bubble formed by the closing up of this tube, weighed down in the centre as in Figs. 5 and 6 of Series V. Similar results were obtained with other liquids, such as petroleum and alcohol.

It is easy to show in a very striking manner the paramount influence of the condition of the solid surface. I have here a number of similar marbles; this set has been well polished by rubbing with wash leather. I drop them one by one through a space of about 1 foot into this deep, wide, cylindrical glass vessel, lighted up by a lamp placed behind it. You see each marble enters noiselessly and with hardly a visible trace of splash. Now I pick them out and drop them in again (or to save trouble, I drop in these other wet ones), everything is changed. You see how the air is carried to the very bottom of the vessel, and you hear the "*φλοισβος*" of the bubbles as they rise to the surface and burst. These dry but rough marbles behave in much the same way.

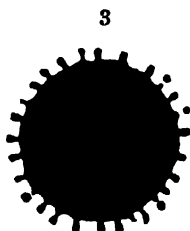
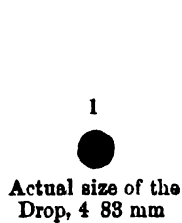
Such are the main features of the Natural History of Splashes, as I made it out between thirteen and eighteen years ago. Before passing on to the photographs that I have since obtained, I desire to add a few words of comment. I have not till now alluded to any imperfections in the timing apparatus. But no apparatus of the kind can be absolutely perfect, and as a matter of fact, when everything is adjusted

so as to display a particular stage, it will happen that in a succession of observations there is a certain variation in what is seen. Thus the configuration viewed may be said to oscillate slightly about the mean for which the apparatus is adjusted. Now this is due both to small imperfections in the timing apparatus and to the fact that the splashes themselves do actually vary within certain limits. The reasons are not very far to seek. In the first place the rate of demagnetisation of the electromagnets varies slightly, being partly dependent on the varying resistance of the contacts of crossed wires, partly on the temperature of the magnet, which is affected by the length of time for which the current has been running. But a much more important reason is the variation of the slight adhesion of the drop to the smoked watch-glass that has supported it, and consequently of the oscillations to which, as we shall see, the drop is subjected as it descends. Thus the drop will sometimes strike the surface in a flattened form, at others in an elongated form, and there will be a difference, not only in the time of impact, but in the nature of the ensuing splash; consequently some judgment is required in selecting a consecutive series of drawings. The only way is to make a considerable number of drawings of each stage, and then to pick out a consecutive series. Now, whenever judgment has to be used, there is room for error of judgment, and moreover, it is impossible to put together the drawings so as to tell a consecutive story, without being guided by some theory, such as I have already sketched, as to the nature of the motion and the conditions that govern it. You will therefore be good enough to remember that this chronicle of the events of a tenth of a second is presented by a fallible human historian, whose account, like that of any other contemporary observer, will be none the worse for independent confirmation. That confirmation I am fortunately able in some measure to supply. When I endeavoured eighteen years ago to photograph the splash of a drop of mercury, I was unable to obtain plates sufficiently sensitive to respond to the very short exposures that were required, and consequently abandoned the attempt. But in recent years plates of exquisite sensitiveness have been produced, and such photographs as those taken by Mr. Boys of a flying rifle bullet, have shown that difficulties on the score of sensitiveness have been practically overcome. Within the last few weeks, with the valuable assistance of my colleague at Devonport, Mr. R. S. Cole, I have succeeded in obtaining photographs of various splashes. Following Prof. Boys' suggestion, we employed Thomas's cyclist plates, or occasionally the less sensitive "extra-rapid" plates of the same makers, and as a developer, Eikonogen solution of triple strength, in which the plates were kept for about 40 minutes, the development being conducted in complete darkness.

A few preliminary trials with the self-induction spark produced at the surface of mercury by the apparatus that you have seen at work, showed that the illumination, though ample for direct vision, was not

# SERIES X.

(1) *Instantaneous Shadow Photographs (life size) of the Splash of a Drop of Mercury falling 8 cm. on to the Photographic Plate.*





SERIES XI.

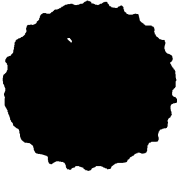
(2) *Instantaneous Shadow Photographs (life size) of the Splash of a Drop of Mercury falling 15 cm. on to Glass.*

1



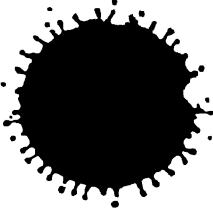
Actual size, 4.83 mm.  
in diameter.

2

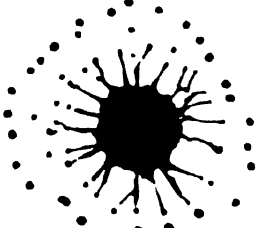


$\tau = 0$

3

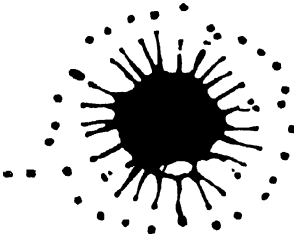


4

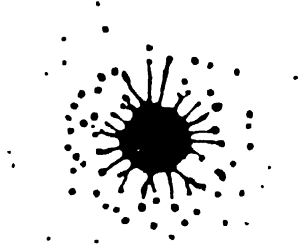


$\tau = .0032$

4A

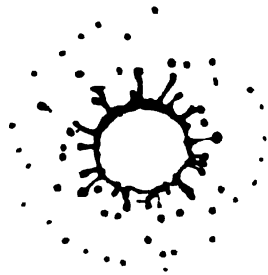


5



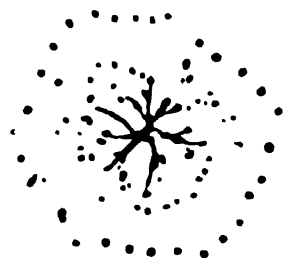
$\tau = .0063$

5A



$\tau = .0094$

6



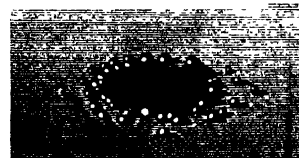
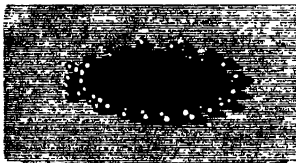
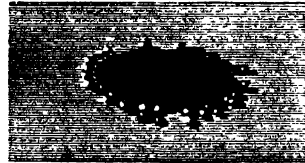
$\tau = .0134$

## SERIES XII.

*Engravings from Instantaneous Photographs ( $\frac{1}{7}$  of the real size) of the Splash of a Drop of Mercury, 4.83 mm. in diameter, falling 8.9 cm. on to a hard polished surface.*



$\tau = 0$



$\tau = .0195 \text{ sec.}$

sufficient for photography. When the current strength was increased, so as to make the illumination bright enough for the camera, then the spark became of too great duration, for it lasted for between 4 and 5 thousandths of a second, within which time there was very perceptible motion of the drop and consequent blurring. It was therefore necessary to modify the apparatus so as to employ a Leyden-jar spark whose duration was probably less than 10-millionths of a second. A very slight change in the apparatus rendered it suitable for the new conditions, but time does not permit me to describe the arrangements in detail. It is, however, less necessary to do so as the method is in all essentials the same as that described in this room two years ago by Lord Rayleigh in connection with the photography of a breaking soap-film.\* I therefore pass at once to the photographs themselves.

The first two series (X. and XI.) may be described as shadow photographs; they were obtained by allowing a drop of mercury to fall on to the naked photographic plate itself, the illuminating spark being produced vertically above it, and they give only a horizontal section of the drop in various stages. The first series corresponds to a mercury splash very similar to that first described, and the second to the splash of a larger drop such as was not described. In each series, the tearing of the thin central film to which allusion was made is well illustrated. I think the first comment that any one would make is that the photographs, while they bear out the drawings in many details, show greater irregularity than the drawings would have led one to expect. On this point I shall presently have something to say.

Comparing the first set of drawings with the photographs of Series X. it will be seen that

Photograph 2 corresponds to drawing 4 or 5,

"	3	"	"	9
"	4	"	"	18
"	6	"	"	20
"	7	"	"	24

but the irregularity of the last photograph almost masks the resemblance.

Series XII. gives an objective view of a mercury splash as taken by the camera. Only the first of this series shows any detail in the interior. The polished surface of the mercury is, in fact, very troublesome to illuminate, and this splash proved the most difficult of all to photograph.

Series XIII. shows the splash of a drop of milk falling on to a smoked glass plate, on which it runs about without adhesion just as

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\* A detailed account of the optical, mechanical, and electrical arrangements employed, written by Mr. Cole, will be found in 'Nature,' vol. 1. p. 222 (July 5, 1894).

mercury would. Here there is much more of detail. In Fig. 4 the central film is so thin in the middle that the black plate beneath it is seen through the liquid. In Fig. 8 this film has been torn.

Series XIII. exhibits the splash of a water drop falling into milk. The first four photographs show the oscillations of the drop about a mean spherical figure as it approaches the surface.

In the subsequent figures it will be noticed that the arms which are thrown up at first, afterwards segment into drops which fly off and subside (see Fig. 8), to be followed by a second series which again subside (Fig. 11), to be again succeeded by a third set. In fact, so long as there is any downward momentum the drop and the air behind it are penetrating the liquid, and so long must there be an upward flow of displaced liquid. Much of this flow is seen to be directed into the arms along the channels determined by the segmentation of the annular rim. This reproduction of the lobes and arms time after time on a varying scale goes far to explain the puzzling variations in their number which I mentioned in connection with the drawings. I had not, indeed, suspected this, which is one of the few new points that the photographs have so far revealed.

With respect to these photographs,\* the credit of which I hope you will attribute firstly to the inventors of the sensitive plates, and secondly to the skill and experience of Mr. Cole, I desire to add that they are, as far as we know, the first really detailed objective views that have been obtained with anything approaching so short an exposure.

Even Mr. Boys' wonderful photographs of flying bullets were after all but shadow-photographs, and did not so strikingly illustrate the extreme sensitiveness of the plates, and I want you to distinguish between such and what (to borrow Mr. F. J. Smith's phrase) I call an "objective view."

It remains only to speak of the greater irregularity in the arms and rays as shown by the photographs. The point is a curious and interesting one. In the first place I have to confess that in looking over my original drawings I find records of many irregular or unsymmetrical figures, yet in compiling the history it has been inevitable that these should be rejected, if only because identical irregularities never recur. Thus the mind of the observer is filled with an ideal splash—an "Auto-Splash"—whose perfection may never be actually realised.

But in the second place, when the splash is nearly regular it is very difficult to detect irregularity. This is easily proved by projecting on the screen with instantaneous illumination such a photo-

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\* Three of these photographs, viz Nos 11, 12 and 17, are reproduced full size, as a frontispiece, by a *photographic* process, to enable the reader to form a more correct idea than can be gathered from the engravings, of the amount of detail actually obtained.

The black streaks seen in Figs. 11, 15, 16 and 17 are due to particles of lamp-black carried down by the drop from the smoked surface on which it rested.

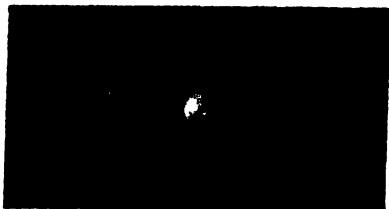
# SERIES XIII.

*Engravings of Instantaneous Photographs ( $\frac{1}{17}$  of the real size) of the Splash of a Drop of Milk falling 20 cm. on to smoked glass.*

1



2



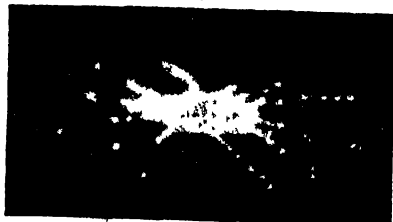
$\tau = 0$

3



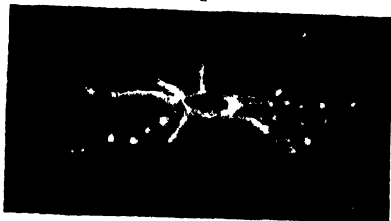
$\tau = .0025 \text{ sec.}$

7



$\tau = .0128 \text{ sec.}$

8



9



$\tau = .0149 \text{ sec.}$

(It was not found possible to reproduce satisfactorily the missing figures of this series.)

SERIES XIV.

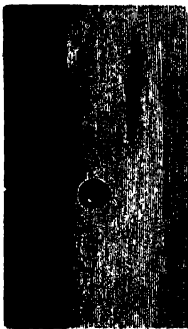
*Engravings of Instantaneous Photographs of the Splash of a Drop of Water falling 40 cm. into Milk.*

Scale about  $\frac{1}{10}$  of actual size.

1



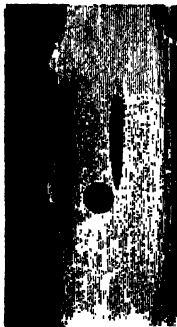
2



3



4



$\tau = 0$

5



6



$\tau = .0056 \text{ sec.}$

7



$\tau = .0163 \text{ sec.}$

8



9



$\tau = .0182 \text{ sec.}$

10

 $\tau = \cdot 0197 \text{ sec.}$ 

13

 $\tau = \cdot 0514 \text{ sec.}$ 

16

 $\tau = \cdot 080 \text{ sec.}$ 

11

 $\tau = \cdot 0262 \text{ sec.}$ 

14

 $\tau = \cdot 0601 \text{ sec.}$ 

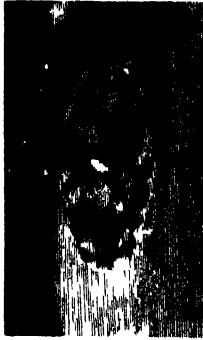
17



12

 $\tau = \cdot 0391 \text{ sec.}$ 

15



18



graph as that of Series X. Fig. 6. My experience is that most persons pronounce what they have seen to be a regular and symmetrical star-shaped figure, and they are surprised when they come to examine it by detail in continuous light to find how far this is from the truth. Especially is this the case if no irregularity is suspected beforehand. I believe that the observer, usually finding himself unable to attend to more than a portion of the rays in the system, is liable instinctively to pick out for attention a part of the circumference where they are regularly spaced, and to fill up the rest in imagination, and that where a ray may be really absent he prefers to consider that it has been imperfectly viewed.

This opinion is confirmed by the fact that in several cases I have been able to observe with the naked eye a splash that was also simultaneously photographed, and have made the memorandum "quite regular," though the photograph subsequently showed irregularity. It must, however, be observed that the absolute darkness and other conditions necessary for photography are not very favourable for direct vision.

And now my tale is told, or rather as much of it as the limits of the time allowed me will permit. I think you will agree that the phenomena are very beautiful, and that the details of this transaction, familiar though it has been to all mankind since the world began, have yet proved worthy of an hour's attention.

[A. M. W.]



Friday, June 1, 1894.

LUDWIG MOND, Esq. F.R.S. Vice-President, in the chair.

PROFESSOR OLIVER LODGE, D.Sc. LL.D. F.R.S.

*The Work of Hertz.\**

THE untimely end of a young and brilliant career cannot fail to strike a note of sadness and awaken a chord of sympathy in the hearts of his friends and fellow-workers. Of men thus cut down in the early prime of their powers there will occur to us here the names of Fresnel, of Carnot, of Clifford, and now of Hertz. His was a strenuous and favoured youth; he was surrounded from his birth with all the influences that go to make an accomplished man of science—accomplished both on the experimental and on the mathematical side. The front rank of scientific workers is weaker by his death, which occurred on January 1, 1894, the thirty-seventh year of his life. Yet did he not go till he had effected an achievement which will hand his name down to posterity as the founder of an epoch in experimental physics.

In mathematical and speculative physics others had sown the seed. It was sown by Faraday, it was sown by Thomson and by Stokes, by Weber also doubtless, and by Helmholtz; but in this particular department it was sown by none more fruitfully and plentifully than by Clerk Maxwell. Of the seed thus sown Hertz reaped the fruits. Through his experimental discovery, Germany awoke to the truth of Clerk Maxwell's theory of light, of light and electricity combined, and the able army of workers in that country (not forgetting some in Switzerland, France and Ireland) have done most of the gleaning after Hertz.

This is the work of Hertz which is best known, the work which brought him immediate fame. It is not always that public notice is so well justified. The popular instinct is generous and trustful, and it is apt to be misled. The scientific eminence accorded to a few energetic persons by popular estimate is more or less amusing to those working on the same lines. In the case of Hertz no such mistake has been made. His name is not over well known, and his work is

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\* The illustrations in this abstract appeared, after the delivery of the discourse, in a little book called "The Work of Hertz and some of his Successors," by Professor Lodge, and are inserted here by the kind permission of the proprietors of the *Electrician*.

immensely greater in every way than that of several who have made more noise.

In closing these introductory and personal remarks, I should like to say that the enthusiastic admiration for Hertz's spirit and character, felt and expressed by students and workers who came into contact with him, is not easily to be exaggerated. Never was a man more painfully anxious to avoid wounding the susceptibilities of others; and he was accustomed to deprecate the prominence given to him by speakers and writers in this country, lest it might seem to exalt him unduly above other and older workers among his own sensitive countrymen.

Speaking of the other great workers in physics in Germany, it is not out of place to record the sorrow with which we have heard of the recent death of Dr. August Kundt, Professor in the University of Berlin, successor to Von Helmholtz in that capacity.

When I consented to discourse on the work of Hertz, my intention was to repeat some of his actual experiments, and especially to demonstrate his less known discoveries and observations. But the fascination exerted upon me by electric oscillation experiments, when I, too, was independently working at them in the spring of 1888,\* resumed its hold, and my lecture will accordingly consist of experimental demonstrations of the outcome of Hertz's work rather than any precise repetition of portions of that work itself.

In case a minority of my audience are in the predicament of not knowing anything about the subject, a five minutes' explanatory prelude may be permitted, though time at present is very far from being "infinitely long."

The simplest way will be for me hastily to summarise our knowledge of the subject before the era of Hertz.

Just as a pebble thrown into a pond excites surface ripples, which can heave up and down floating straws under which they pass, so a struck bell or tuning-fork emits energy into the air in the form of what are called sound waves, and this radiant energy is able to set up vibrations in other suitable elastic bodies.

If the body receiving them has its natural or free vibrations violently damped, so that when left to itself it speedily returns to rest, Fig. 1, then it can respond fully to notes of almost any pitch. This is the case with your ears and the tones of my voice. Tones must be exceedingly shrill before they cease to excite the ear at all.

If, on the other hand, the receiving body has a persistent period of vibration, continuing in motion long after it is left to itself, Fig. 2, like another tuning fork or bell, for instance, then far more facility of response exists, but great accuracy of tuning is necessary if it is to be fully called out; for if the receiver is not thus accurately syntonised with the source, it fails more or less completely to resound.

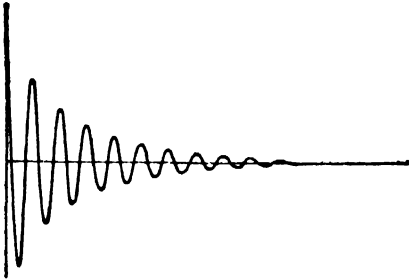
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\* Phil Mag, xxvi. pp. 229, 230, August 1888; or "Lightning Conductors and Lightning Guards" (Whittaker), pp. 104, 105; also Proc. Roy. Soc., vol. 50, p. 27.

Conversely, if the *source* is a persistent vibrator, correct tuning is essential, or it will destroy at one moment, Fig. 3, motion which it originated the previous moment. Whereas, if it is a dead-beat or strongly-damped exciter, almost anything will respond equally well or equally ill to it.

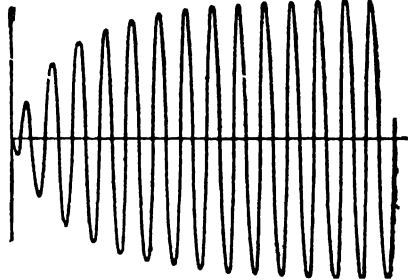
What I have said of sounding bodies is true of all vibrators in a medium competent to transmit waves. Now a sending telephone or a

FIG. 1.



Oscillations of Dumb-bell Hertz  
Vibrator (after Bjerknes).

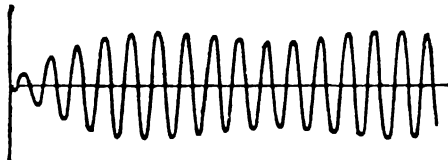
FIG. 2.



Oscillation of Ring-shaped Hertz  
Resonator excited by the syn-  
tonic Vibrator which gave the  
curve Fig. 1 (after Bjerknes).

microphone, when spoken to, emits waves into the ether, and this radiant energy is likewise able to set up vibration in suitable bodies. But we have no delicate means of directly detecting these electrical or ethereal waves; and if they are to produce a perceptible effect at a distance, they must be confined, as by a speaking-tube, prevented from spreading, and concentrated on the distant receiver.

FIG. 3.



Oscillation of Ring Resonator similarly excited but not quite syntonized with  
Radiator. (For method of obtaining these curves see Fig. 14.)

This is the function of the telegraph wire; it is to the ether what a speaking-tube is to air. A metal wire in air (*in function*, not in details of analogy) is like a long hollow cavity surrounded by nearly rigid but slightly elastic walls.

Furthermore, any conductor electrically charged or discharged with sufficient suddenness must emit electrical waves into the ether, because the charge given to it will not settle down instantly, but will

surge to and fro several times first; and these surgings or electric oscillations must, according to Maxwell, start waves in the ether, because at the end of each half-swing they cause electrostatic, and at the middle of each half-swing they cause electromagnetic effects, and the rapid alternation from one of these modes of energy to the other constitutes ethereal waves.\* If a wire is handy they will run along it, and may be felt a long way off. If no wire exists they will spread out like sound from a bell, or light from a spark, and their intensity will decrease according to the inverse square of the distance.

Maxwell and his followers well knew that there would be such waves; they knew the rate at which they would go, they knew that they would go slower in glass and water than in air, they knew that they would curl round sharp edges, that they would be partly absorbed but mainly reflected by conductors, that if turned back upon themselves they would produce the phenomena of stationary waves, or interference, or nodes and loops; it was known how to calculate the length of such waves, and even how to produce them of any required or predetermined wave-length from 1000 miles to a foot. Other things were known about them which would take too long to enumerate; any homogeneous insulator would transmit them, would refract or concentrate them if it were of suitable shape, would reflect none of a particular mode of vibration at a certain angle, and so on, and so on.

All this was *known*, I say, known with varying degrees of confidence; but by some known with as great confidence as, perhaps even more confidence than, is legitimate before the actuality of experimental verification.

Hertz supplied the verification. He inserted suitable conductors in the path of such waves, conductors adapted for the occurrence in them of induced electric oscillations, and to the surprise of every one, himself, doubtless, included, he found that the secondary electric surgings thus excited were strong enough to display themselves by minute electric sparks.

I shall show this in a form which requires great precision of tuning, or syntonny, both emitter and receiver being persistently vibrating things giving some 30 or 40 swings before damping has a serious effect. I take two Leyden jars with circuits about a yard in diameter, and situated about two yards apart, Fig. 4. I charge and discharge one jar, and observe that the surgings set up in the other can cause it to overflow if it is syntonised with the first.†

A closed circuit such as this is a feeble radiator and a feeble

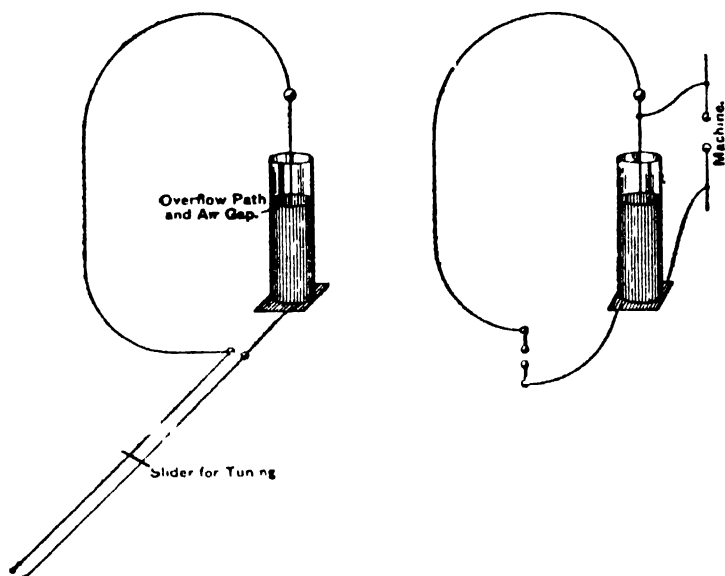
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\* Strictly speaking, in the waves themselves there is no lag or difference of phase between the electric and the magnetic vibrations; the difference exists in emitter or absorber, but not in the transmitting medium. True radiation of energy does not begin till about a quarter wave-length from the source, and within that distance the initial quarter period difference of phase is obliterated.

† See 'Nature,' vol. xli. p 368; or J. J. Thomson, 'Recent Researches,' p. 395.

absorber, so it is not adapted for action at a distance. In fact, I doubt whether it will visibly act at a range beyond the  $\frac{1}{4} \lambda$  at which true radiation of broken-off energy occurs. If the coatings of the jar are separated to a greater distance, so that the dielectric is more

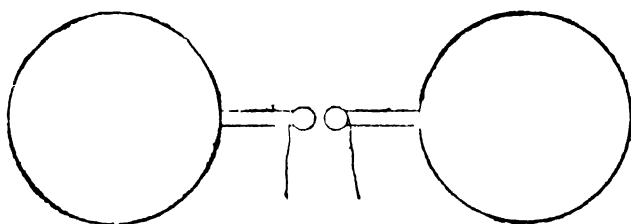
FIG. 4.



Experiment with syntonized Leyden Jars.

exposed, it radiates better; because in true radiation the electrostatic and the magnetic energies are equal, whereas in a ring circuit the magnetic energy greatly predominates. By separating the coats of the jar

FIG. 5.



Standard Hertz Radiator.

as far as possible we get a typical Hertz vibrator, Fig. 5, whose dielectric extends out into the room, and this radiates very powerfully.

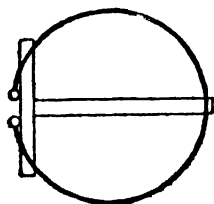
In consequence of its radiation of energy, its vibrations are rapidly damped, and it only gives some three or four good strong

swings, Fig. 1. Hence it follows that it has a wide range of excitation; i. e. it can excite sparks in conductors barely at all in tune with it.

The two conditions, conspicuous energy of radiation and persistent vibration electrically produced, are at present incompatible.

Whenever these two conditions coexist, considerable power or activity will, of course, be necessary in the source of energy. At present they only coexist in the sun and other stars, in the electric arc and in furnaces.

FIG. 6.

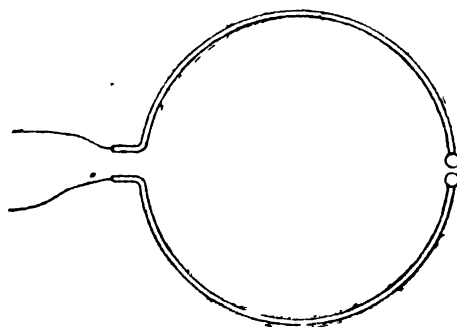


Circular Resonator.  
(The knobs ought to nearly touch each other.)

The receiver Hertz used was chiefly a circular resonator, Fig. 6, not a good absorber but a persistent vibrator, well adapted for picking up disturbances of precise and measurable wavelength. Its mode of vibration when excited by an emitter in tune with it is depicted in Fig. 2. I find that the circular resonators can act as senders too; here is one (Fig. 6A) exciting quite long sparks in a second one.

*Electric Syntony*:—that was his discovery, but he did not stop there. He at once proceeded to apply his discovery to the verification of what had already been predicted about the waves, and by laborious and difficult interference experiments he ascertained that the

FIG. 6A.



Any circular resonator can be used as a sender by bringing its knobs near the sparking knobs of a coil; but a simple arrangement is to take two semicircles, as in above figure, and make them the coil terminals. The capacity of the cut ends can be varied, and the period thereby lengthened, by expanding them into plates.

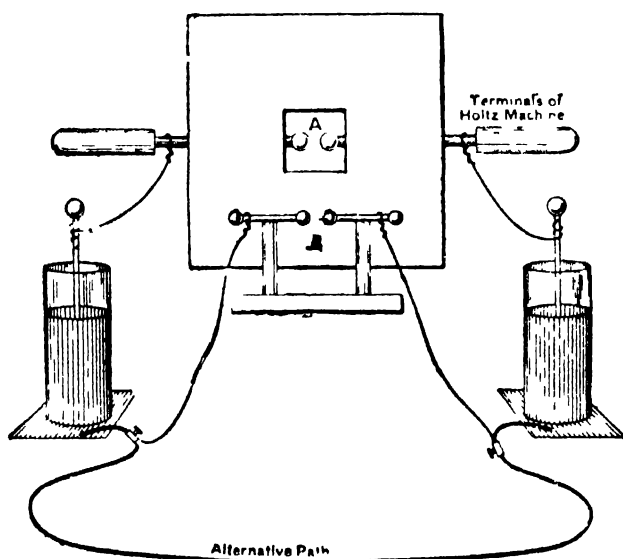
previously calculated length of the waves was thoroughly borne out by fact. These interference experiments in free space are his greatest achievement.

He worked out every detail of the theory splendidly, separately analysing the electric and the magnetic oscillation, using language

not always such as we should use now, but himself growing in theoretic insight through the medium of what would have been to most physicists a confusing maze of troublesome facts, and disentangling all their main relations most harmoniously.

While Hertz was observing sparks such as these, the primary or exciting spark and the secondary or excited one, he observed as a by-issue that the secondary spark occurred more easily if the light from the primary fell upon its knobs. He examined this new influence of light in many ways, and showed that although spark light and electric brush light were peculiarly effective, any source of light that gave very ultra-violet rays produced the same result.

FIG. 7.



Experiment arranged to show effect on one spark of light from another. The B spark occurs more easily when it can see the A spark through the window, unless the window is glazed with glass. A quartz pane transmits the effect.

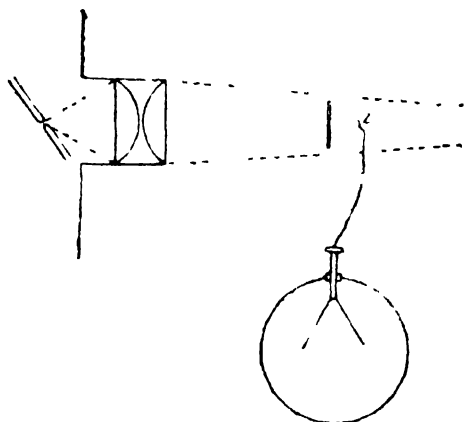
The above figure represents my way of showing the experiment. It will be observed that with this arrangement the B knobs are at the same potential up to the instant of the flash, and in that case the ultra-violet portion of the light of the A spark assists the occurrence of the B spark. But it is interesting to note, what Elster and Geitel have found, that if the B knobs were subjected to steady strain instead of to impulsive rush—e. g. if they were connected to the inner coats of the jars instead of to the outer coatings—that then the effect of ultra-violet light on either spark-gap would exert a deterrent influence, so that the spark would probably occur at the other, or non-illuminated

gap. With these altered connections it is, of course, not feasible to illuminate one spark by the light of the other; the sparks are then alternative, not successive.

Wiedemann and Ebert, and a number of experimenters, have repeated and extended this discovery, proving that it is the cathode knob on which illumination takes effect; and Hallwachs and Righi independently made the important observation, which Elster and Geitel, Stoletow, Branly and others have extended, that a freshly-polished zinc or other oxidisable surface, if charged negatively, is gradually discharged by ultra-violet light.

It is easy to fail in reproducing this experimental result if the right conditions are not satisfied; but if they are it is absurdly easy, and the thing might have been observed nearly a century ago.

FIG. 8.



Zinc Knob in Arc Light, protected by Glass Screen. The lenses are of quartz, but there is no need for any lenses in this experiment; leakage begins directly the glass plate is withdrawn.

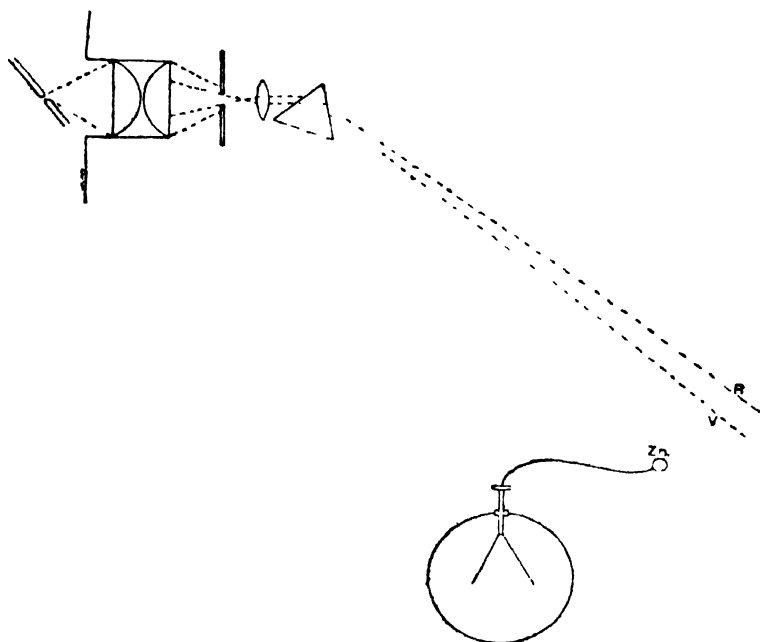
Take a piece of zinc, clean it with emery paper, connect it to a gold leaf electroscope, and expose it to an arc lamp, Fig. 8. If charged positively nothing appears to happen, the action is very slow; but a negative charge leaks away in a few seconds if the light is bright. Any source of light rich in ultra-violet rays will do; the light from a spark is perhaps most powerful of all. A pane of glass cuts off all the action; so does atmospheric air in sufficient thickness (at any rate, town air), hence sunlight is not powerful. A pane of quartz transmits the action almost undiminished, but fluor-spar may be more transparent still. Condensing the arc rays with a quartz lens and analysing them with a quartz prism or reflection grating, we find that the most effective part of the light is high up in the ultra-



violet, surprisingly far beyond the limits of the visible spectrum \* (Fig. 9).

This is rather a digression, but I have taken some pains to show it properly because of the interest betrayed by Lord Kelvin on this

FIG. 9.



Zinc Knob discharging Negative Electricity in the very Ultra-violet Light of a Spectrum formed by a Quartz Train. Right position of Knob shown.

matter, and the caution which he felt about accepting the results of the Continental experimenters too hastily.

It is probably a chemical phenomenon, and I am disposed to

\* While preparing for the lecture it occurred to me to try, if possible during the lecture itself, some new experiments on the effect of light on negatively charged bits of rock and ice, because if the effect is not limited to metals it must be important in connection with atmospheric electricity. When Mr. Branly coated an aluminium plate with an insulating varnish, he found that its charge was able to soak in and out of the varnish during illumination ('Comptes Rendus,' vol. cx. p. 898, 1890). Now, the mountain tops of a negatively charged earth are exposed to very ultra-violet rays, and the air is a dielectric in which quiet up-carrying and sudden downpour of electricity could go on in a manner not very unlike the well-known behaviour of water vapour; and this, perhaps, may be the reason, or one of the reasons, why it is not unusual to experience a thunderstorm

express it as a modification of the Volta contact effect \* with illumination.

Return now to the Hertz vibrator, or Leyden jar with its coatings well separated, so that we can get into its electric as well as its magnetic field. Here is a great one giving waves 30 metres long, radiating while it lasts with an activity of 100 H.P., and making ten million complete electric vibrations per second (Fig. 10).

Its great radiating power damps it down very rapidly, so that it does not make above two or three swings; but nevertheless, each time it is excited, sparks can be drawn from most of the reasonably elongated conductors in this theatre.

A suitably situated gas-leak can be ignited by these induced sparks. An Abel's fuse connecting the water pipes with the gas

FIG. 10.



Large Hertz Oscillator on reduced scale,  $\frac{1}{16}$  inch to a foot.

pipes will blow off; vacuum tubes connected to nothing will glow (this fact has been familiar to all who have worked with Hertz waves since 1889); electric leads, if anywhere near each other, as they are in some incandescent lamp holders, may spark across to each other, thus striking an arc and blowing their fuses. This blowing of fuses by electric radiation frequently happened at Liverpool till the suspensions of the theatre lamps were altered.

The striking of an arc by the little reverberating sparks between two lamp-carbons connected with the 100-volt mains I incidentally

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after a few fine days. I have now tried these experiments on such geological fragments as were handy, and find that many of them discharge negative electricity under the action of a naked arc, especially from the side of the specimens which was somewhat dusty, but that when wet they discharge much less rapidly, and when positively charged hardly at all. Ice and garden soil discharge negative electrification, too, under ultra-violet illumination, but not so quickly as limestone, mica schist, ferruginous quartz, clay and some other specimens. Granite barely acts: it seems to insulate too well. The ice and soil were tried in their usual moist condition, but, when thoroughly dry, soil discharges quite rapidly. No rock tested was found to discharge as quickly as does a surface of perfectly bright metal, such as iron, but many discharged much more quickly than ordinary dull iron, and rather more quickly than when the bright iron surface was thinly oiled or wetted with water. To-day (June 5) I find that the leaves of *Geranium* discharge positive electrification five times as quickly as negative, under the action of an arc-light, and that glass cuts the effect off while quartz transmits it.

[*Added later.* Arrhenius has had the same notion about atmospheric electricity; and Elster and Geitel have made elaborate and careful experiments on the subject. Wied. Ann. vols. xxxix., xl., xli., &c.]

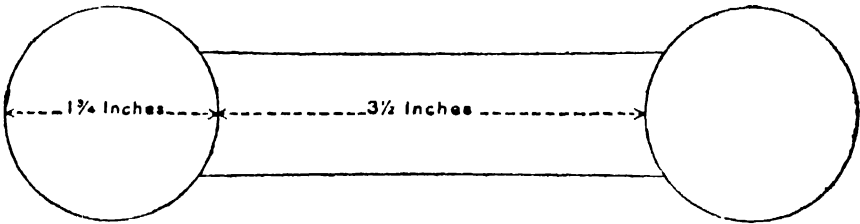
\* See Brit. Assoc. Report, 1884, pp. 502-519, or Phil. Mag., vol. xix. pp. 267-352.

now demonstrate. An arc is started directly the large Hertz vibrator is excited at a distance.

There are some who think that lightning flashes can do none of these secondary things. They are mistaken.

On the table are specimens of various emitters and receivers such as have been used by different people; the orthodox Hertz radiator

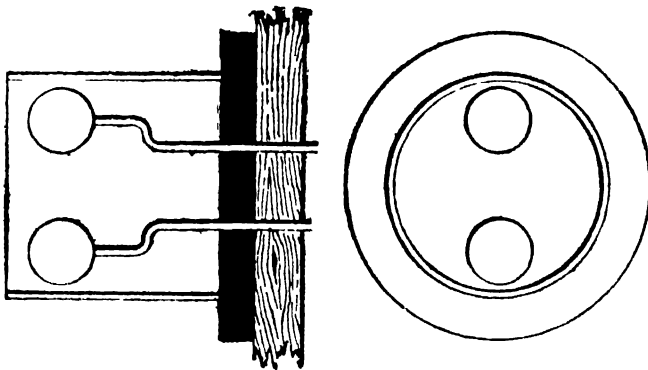
FIG. 11.



A Dumb-bell Form of Radiator.

of dumb-bell type, Fig. 5, and the orthodox Hertz receivers:—circular ring, Fig. 6, for interference experiments, because it is but little damped, and a straight wire for receiving at a distance, because it is a much better absorber. Beside these are the spheres and ellipsoids (or elliptical plates), which I have mainly used, Fig. 19, because they are powerful radiators and absorbers, and because their theory

FIG. 12.



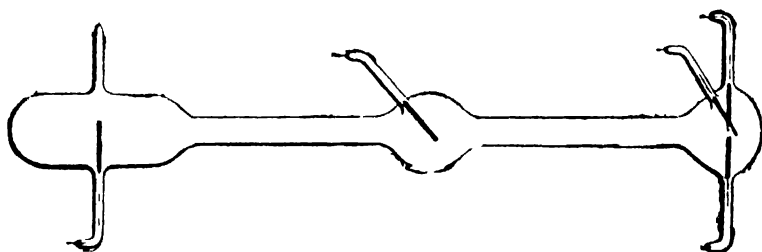
Dr. Lodge's Hollow Cylindrical Radiator, arranged horizontally against the outside of a metal-lined box. Half natural size. Emitting 3-inch waves.

has been worked out by Horace Lamb and J. J. Thomson. Also dumb-bells, Fig. 11, without air-gap, and many other shapes, the most recent of mine being the inside of a hollow cylinder with sparks at ends of a diameter, Fig. 12; this being a feeble radiator, but a very

persistent vibrator,\* and, therefore, well adapted for interference and diffraction experiments. But, indeed, spheres can be made to vibrate longer than usual by putting them into copper hats or enclosures, in which an aperture of varying size can be made to let the waves out, Figs. 20 and 21.

Many of these senders will do for receivers too, giving off sparks to other insulated bodies or to earth; but, besides the Hertz type of receiver, many other detectors of radiation have been employed. Vacuum tubes can be used, either directly or on the trigger principle, as by Zehnder, Fig. 13,† the resonator spark precipitating a discharge from some auxiliary battery or source of energy, and so making a feeble disturbance very visible. Explosives may be used for the same purpose, either in the form of mixed water-gases or in the form of an Abel's fuse. Fitzgerald found that a tremendously sensitive

FIG. 13.



Zehnder's Trigger Tube. Half natural size. The two right-hand terminals, close together, are attached to the Hertz receiver; another pair of terminals are connected to some source just not able to make the tube glow until the scintilla occurs and makes the gas more conducting—as observed by Schuster and others.

galvanometer could indicate that a feeble spark had passed, by reason of the consequent disturbance of electrical equilibrium which settled down again through the galvanometer.‡ This was the method he used in this theatre four years ago. Blyth used a one-sided electrometer, and V. Bjerknes has greatly developed this method, Fig. 14, abolishing the need for a spark, and making the electrometer metrical, integrating and satisfactory.§ With this detector many measurements have been made at Bonn by Bjerknes, Yule, Barton, and others, on waves concentrated and kept from space dissipation by guiding wires.

Mr. Boys has experimented on the mechanical force exerted by electrical surgings, and Hertz also made observations of the same kind.

Going back to older methods of detecting electrical radiation, we have, most important of all, a discovery made long before man

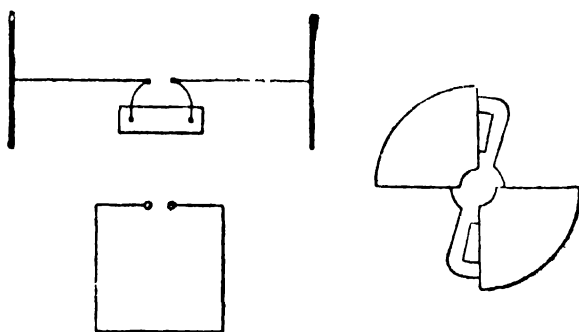
\* J. J. Thomson, 'Recent Researches,' 344. † Wied. Ann., xlvii. p. 77.

‡ Fitzgerald, 'Nature,' vol. xli. p. 295, and vol. xlii. p. 172.

§ Wied. Ann., xlv. p. 74.

existed, by a creature that developed a sensitive cavity on its skin; a creature which never so much as had a name to be remembered by (though perhaps we now call it trilobite). Then, in recent times we recall the photographic plate and the thermopile, with its modification, the radiomicrotometer; also the so-called bolometer, or otherwise-known Siemens' pyrometer, applied to astronomy by Langley, and applied to the detection of electric waves in wires by Rubens and Ritter and Paalzow and Arons. The thermal junction was applied to the same purpose by Klemencik, D. E. Jones and others.

FIG. 14.



Bjerknæs' Apparatus, showing (1) a Hertz vibrator connected to an induction coil; (2) a nearly-closed circuit receiver properly tuned with the vibrator; and (3) a one-sided electrometer for inserting in the air-gap of 2. The receiver is not provided with knobs, as shown, but its open circuit is terminated by the quadrants of the electrometer, which is shown on an enlarged scale alongside. The needle is at zero potential and is attracted by both quadrants. By calculation from the indications of this electrometer Bjerknæs plotted the curves 1, 2 and 3 on page 329. Fig. 1 represents the oscillations of the primary vibrator, rapidly damped by radiation of energy. Fig. 2 represents the vibrations thereby set up in the resonating circuit when the two are accurately in tune; and which persist for many swings. Fig. 3 shows the vibrations excited in the same circuit when slightly out of tune with the exciter. A receiver of this kind makes many swings before it is seriously damped.

And, before all these, the late Mr. Gregory, of Cooper's Hill, made his singularly sensitive expansion meter, whereby waves in free space could be detected by the minute rise of temperature they caused in a platinum wire, a kind of early and sensitive form of Cardew voltmeter. Boys, Briscoe and Watson developed this method.

Going back to the physiological method of detecting surgings, Hertz tried the frog's-leg nerve and muscle preparation, which to the steadier types of electrical stimulus is so surpassingly sensitive, and to which we owe the discovery of current electricity. But he failed to get any result. Ritter has succeeded; but in my experience, failure is the normal and proper result. Working with my colleague,

Prof. Gotch, at Liverpool, I too have tried the nerve and muscle preparation of the frog, Fig. 15, and we find that an excessively violent stimulus of a rapidly alternating character, if pure and unaccompanied by secondary actions, produces no effect—no stimulating effect, that is, even though the voltage is so high that sparks are ready to jump between the needles in direct contact with the nerve.

All that such oscillations do, if continued, is to produce a temporary paralysis or fatigue of the nerve, so that it is unable to transmit the nerve impulses evoked by other stimuli, from which paralysis it recovers readily enough in course of time.

This has been expected from experiments on human beings, such experiments as Tesla's and those of d'Arsonval. But an entire animal is not at all a satisfactory instrument wherewith to attack the question; its nerves are so embedded in conducting tissues that it may easily be doubted whether the alternating type of stimulus ever reaches them at all. By dissecting out a nerve and muscle from a deceased frog after the historic manner of physiologists, and applying

FIG. 15.



Experiment of Gotch and Lodge on the physiological effect of rapid pure electric alternations. Nerve-muscle preparation, with four needles, or else non-polarisable electrodes applied to the nerve. C and D are the terminals of momentary rapidly alternating electric current from a conductor at zero potential, while A and B are the terminals of an ordinary very weak galvanic or induction coil stimulus only just sufficient to make the muscle twitch.

the stimulus direct to the nerve, at the same time as some other well known  $\frac{1}{100}$  of a volt stimulus is applied to another part of the same nerve further from the muscle, it can be shown that rapid electric alternations, if entirely unaccompanied by static charge or by resultant algebraic electric transmission, evoke no excitatory response until they are so violent as to give rise to secondary effects such as heat or mechanical shock. Yet, notwithstanding this inaction they gradually and slowly exert a paralysing or obstructive action on the portion of the nerve to which they are applied, so that the nerve impulse excited by the feeble, just perceptible  $\frac{1}{100}$ -volt stimulus above is gradually throttled on its way down to the muscle, and remains so throttled for a time varying from a few minutes to an hour after the cessation of the violence.

Among trigger methods of detecting electric radiation, I have spoken of the Zehnder vacuum tubes; another method is one used by Boltzmann.\* A pile of several hundred volts is on the verge of charging an electroscope through an air gap just too wide to break

\* Wied. Ann., xl p. 399.

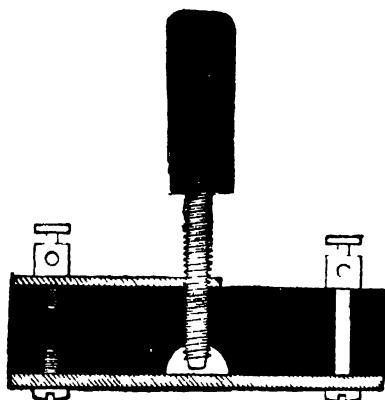
down. Very slight electric surgings precipitate the discharge across the gap, and the leaves diverge. I show this in a modified and simple form. On the cap of an electroscope is placed a highly-polished knob or rounded end connected to the sole, and just not touching the cap, or rather just not touching a plate connected with the cap, Fig. 16, the distance between knob and plate being almost infinitesimal, such a distance as is appreciated in spherometry. Such an electroscope overflows suddenly and completely with any gentle rise of potential. Bring excited glass near it, the leaves diverge gradually and then suddenly collapse, because the air space snaps; remove the glass and they rediverge with negative electricity; the knob above the cap being then charged positively and to the verge of sparking. In this condition any electrical waves, collected if weak by a foot or so of wire projecting from the cap, will discharge the electroscope by exciting surgings in the wire, and so breaking down the air-gap. The chief interest about this experiment seems to me the extremely definite dielectric strength of so infinitesimal an air space. Moreover, it is a detector for Hertz waves that might have been used last century; it might have been used by Benjamin Franklin.

For to excite them no coil or anything complicated is necessary; it is sufficient to flick a metal sphere or cylinder with a silk handkerchief and then discharge it with a well-polished knob. If it is not well polished the discharge is comparatively gradual, and the vibrations are weak; the more polished are the sides of an air-gap, the more sudden is the collapse and the more vigorous the consequent radiation, especially the radiation of high frequency, the higher harmonics of the disturbance.

For delicate experiments it is sometimes well to repolish the knobs every hour or so. For metrical experiments it is often better to let the knobs get into a less efficient but more permanent state. This is true of all senders or radiators. For the generation of the, so to speak, "infra-red" Hertz waves any knobs will do, but to generate the "ultra-violet" high polish is essential.

Receivers or detectors, which for the present I temporarily call microphonic, are liable to respond best to the more rapid vibrations.

FIG. 16.



Air-gap for Electroscope. Natural size. The bottom plate is connected to, and represents, the cap of an electroscope; the "knob" above it, mentioned in text, is the polished end of the screw, whose terminal is connected with the case of the instrument or "earth."

Their sensitiveness is to me surprising, though of course it does not approach the sensitiveness of the eye; at the same time, I am by no means sure that the eye differs from them in kind. It is these detectors that I wish specially to bring to your notice.

Prof. Minchin, whose long and patient work in connection with photo-electricity is now becoming known, and who has devised an instrument more sensitive to radiation than even Boys' radiomicro-meter, in that it responds to the radiation of a star while the radiomicro-meter does not, found some years ago that some of his light-excitable cells lost their sensitiveness capriciously on tapping, and later he found that they frequently regained it again while Mr. Gregory's Hertz-wave experiments were going on in the same room.

These "impulsion-cells," as he terms them, are troublesome things for ordinary persons to make and work with—at least I have never presumed to try—but in Mr. Minchin's hands they are surprisingly sensitive to electric waves.\*

The sensitiveness of selenium to light is known to every one, and Mr. Shelford Bidwell has made experiments on the variations of conductivity exhibited by a mixture of sulphur and carbon.

Nearly four years ago M. Edouard Branly found that a burnished coat of porphyrised copper spread on glass diminished its resistance enormously, from some millions to some hundreds of ohms when it was exposed to the neighbourhood, even the distant neighbourhood, of Leyden jar or coil sparks. He likewise found that a tube of metallic filings behaved similarly, but that this recovered its original resistance on shaking. Dr. Dawson Turner exhibited this fact recently at the Edinburgh meeting of the British Association, and Mr. Croft has shown it to the Physical Society. M. Branly also made pastes and solid rods of filings, in Canada balsam and in sulphur, and found them likewise sensitive.†

With me the matter arose somewhat differently, as an outcome of the air-gap detector employed with an electroscope by Boltzmann. For I had observed in 1889 that two knobs sufficiently close together, far too close to stand any voltage such as an electroscope can show, could, when a spark passed between them, actually cohere; conducting an ordinary bell-ringing current if a single voltaic cell was in circuit; and, if there were no such cell, exhibiting an electromotive force of their own sufficient to disturb a low resistance galvanometer vigorously, and sometimes requiring a faintly perceptible amount of force to detach them. The experiment was described to the Institution of Electrical Engineers,‡ and Prof. Hughes said he had observed the same thing.

Well, this arrangement, which I call a coherer, is the most

\* Phil. Mag., vol. xxxi. p. 223.

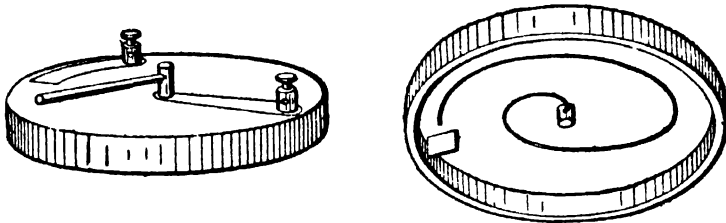
† E. Branly, 'Comptes Rendus,' vol. cxi. p. 785; and vol. cxii. p. 90.

‡ 'Journal' Institution of Electrical Engineers, 1890, vol. xix. pp. 352-4; or 'Lightning Conductors and Lightning Guards' (Whittaker, pp. 382-4.



astonishingly sensitive detector of Hertz waves. It differs from an actual air-gap in that the insulating film is not really insulating; the film breaks down not only much more easily, but also in a less discontinuous and more permanent manner, than an air-gap. A tube of filings, being a series of bad contacts, clearly works on the same plan; and though a tube of filings is by no means so sensitive, yet it is in many respects easier to work with, and, except for very

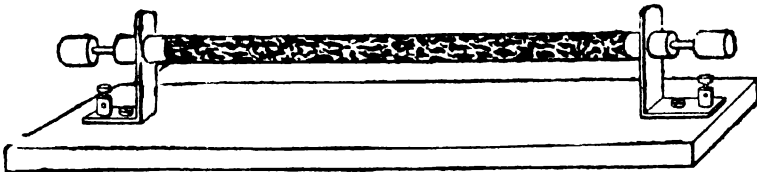
FIG. 17.



Coherer, consisting of a spiral of thin iron wire mounted on an adjustable spindle and an aluminum plate. When the lever is moved clockwise, the tip of the iron wire presses gently against the aluminum plate.

feeble stimuli, is more metrical. If the filings used are coarse, say turnings or borings, the tube approximates to a single coherer; if they are fine, it has a larger range of sensibility. In every case what these receivers feel are sudden jerks of current; smooth sinuous vibrations are ineffective. They seem to me to respond best to waves a few inches long, but doubtless that is determined chiefly by the

FIG. 18.



Iron Borings Tube. One-third natural size

dimensions of some conductor with which they happen to be associated. Figs. 17 and 18.

I picture to myself the action as follows: Suppose two fairly clean pieces of metal in light contact—say two pieces of iron—connected to a single voltaic cell; a film of what may be called oxide intervenes between the surfaces, so that only an insignificant current is allowed to pass, because a volt or two is insufficient to break down

the insulating film, except perhaps at one or two atoms.\* If the film is not permitted to conduct at all, it is not very sensitive; the most sensitive condition is obtained when an infinitesimal current passes, strong enough just to show on a moderate galvanometer.

Now let the slightest surging occur, say by reason of a sphere being charged and discharged at a distance of forty yards; the film at once breaks down—perhaps not completely, that is a question of intensity—but permanently. As I imagine, more molecules get within each other's range, incipient cohesion sets in, and the momentary electric quiver acts somewhat like a flux. It is a singular variety of electric welding. A stronger stimulus enables more molecules to hold on, the process is surprisingly metrical; and, as far as I roughly know at present, the change of resistance is proportional to the energy of the electric radiation, from a source of given frequency.

It is to be specially noted that a battery current is not needed to effect the cohesion, only to demonstrate it. The battery can be applied after the spark has occurred, and the resistance will be found changed as much as if the battery had been on all the time.

The incipient cohesion electrically caused can be mechanically destroyed. Sound vibrations or any other feeble mechanical disturbances, such as scratches or taps, are well adapted to restore the contact to its original high-resistance sensitive condition. The more feeble the electrical disturbance the slighter is the corresponding mechanical stimulus needed for restoration. When working with the radiating sphere, Fig. 19, at a distance of forty yards out of window, I could not for this reason shout to my assistant, to cause him to press the key of the coil and make a spark, but I showed him a duster instead, this being a silent signal which had no disturbing effect on the coherer or tube of filings. I mention 40 Yards, because that was one of the first outdoor experiments; but I should think that something more like half a mile was nearer the limit of sensitiveness. However, this is a rash statement not at present verified. At 40 or 60 yards the exciting spark could be distinctly heard, and it was interesting to watch the spot of light begin its long excursion and actually travel a distance of 2 inches or 3 inches before the sound arrived. This experiment proved definitely enough that the efficient cause travelled quicker than sound, and disposed completely of any sceptical doubts as to sound-waves being, perhaps, the real cause of the phenomenon.

Invariably, when the receiver is in good condition, sound or other mechanical disturbance acts one way, viz. in the direction of increasing resistance, while electrical radiation or jerks act the other way, decreasing it. While getting the receiver into condition, or when it is getting out of order, vibrations and sometimes electric discharges act irregularly; and an occasional good shaking does the

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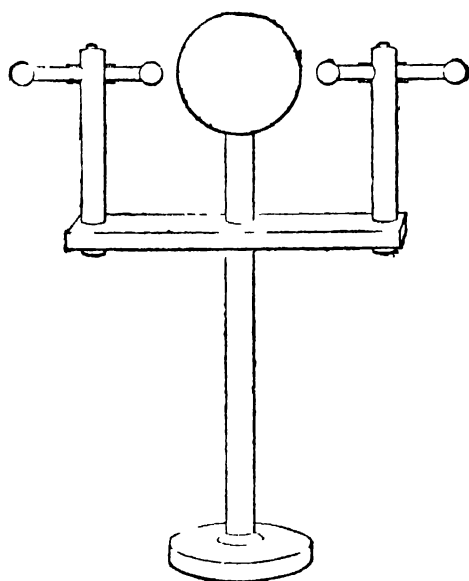
\* See Phil. Mag. Jan. 1894, p. 94.

filings good. I have taken rough measurements of the resistance, by the simple process of restoring the original galvanometer deflection by adding or removing resistance coils. A  $\frac{1}{2}$ -inch tube 8 inches long of selected iron turnings, Fig. 18, had a resistance of 2500 ohms in the sensitive state. A feeble stimulus, caused by a distant electrophorus spark, brought it down 400 ohms. A rather stronger one reduced it by 500 and 600, while a trace of spark given to a point of the circuit itself, ran it down 1400 ohms.

This is only to give an idea of the quantities. I have not yet done any seriously metrical experiments.

From the wall diagram which summarises the various detectors,

FIG. 19.



Radiator used in the library of the Royal Institution, exciting the Coherer (Fig 17) on the lecture table in the theatre. (Sphere 5 inches diameter.)

and which was prepared a month or so ago, I see I have omitted selenium, a substance which in certain states is well known to behave to visible light as these other microphonic detectors behave to Hertz waves. It is now inserted, but with a query to indicate that its position in the table is not *certainly* known.

And I want to suggest that quite possibly the sensitiveness of the eye is of the same kind. As I am not a physiologist I cannot be seriously blamed for making wild and hazardous speculations in that region. I therefore wish to guess that some part of the retina is an electrical organ, say like that of some fishes, maintaining an electromotive force which is prevented from stimulating the nerves

solely by an intervening layer of badly conducting material, or of conducting material with gaps in it; but that when light falls upon the retina these gaps become more or less conducting, and the nerves are stimulated. I do not feel clear which part is taken by the rods and cones, and which part by the pigment cells; I must not try to make the hypothesis too definite at present.

If I had to make a demonstration model of the eye on these lines, I should arrange a little battery to excite a frog's nerve-muscle preparation through a circuit completed all except a layer of filings or a single bad contact. Such an arrangement would respond to Hertz waves. Or, if I wanted actual light to act, instead of grosser waves, I would use a layer of selenium.

But the bad contact and the Hertz waves are the most instructive, because we do not at present really know what the selenium is doing, any more than what the retina is doing.

And observe that (to my surprise, I confess) the rough outline of a theory of vision thus suggested is in accordance with some of the principal views of the physiologist Hering. The sensation of light is due to the electrical stimulus; the sensation of black is due to the mechanical or tapping back stimulus. Darkness is physiologically not the mere cessation of light. Both are positive sensations, and both stimuli are necessary; for until the filings are tapped back vision is persistent. In the eye model the period of mechanical tremor should be, say,  $\frac{1}{10}$  second, so as to give the right amount of persistence of impression.

No doubt in the eye the tapping back is done automatically by the tissues, so that it is always ready for a new impression, until fatigued. And by mounting an electric bell or other vibrator on the same board as a tube of filings, it is possible to arrange so that a feeble electric stimulus shall produce a feeble steady effect, a stronger stimulus a stronger effect, and so on; the tremor asserting its predominance, and bringing the spot back whenever the electric stimulus ceases.

An electric bell thus close to the tube is, perhaps, not the best vibrator; clockwork might do better, because the bell contains in itself a jerky current, which produces one effect, and a mechanical vibration, which produces an opposite effect; hence the spot of light can hardly keep still. By lessening the vibration—say, by detaching the bell from actual contact with the board, the electric jerks of the intermittent current drive the spot violently up the scale; mechanical tremor brings it down again.

You observe that the eye on this hypothesis is, in electrometer language, heterostatic. The energy of vision is supplied by the organism; the light only pulls a trigger. Whereas the organ of hearing is idiostatic. I might draw further analogies between this arrangement and the eye, e. g. about the effect of blows or disorder causing irregular conduction, and stimulation, of the galvanometer in the one instrument, of the brain cells in the other.

## DETECTORS OF RADIATION.

Physiological.	Chemical.	Thermal.	Electrical.	Mechanical.	Microphonic.
Eye.	Photographic Plate.	Thermopile.	Spark. (Hertz.)	Electrometer. (Blyth and Bjerknes.)	Selenium. (?)
× Frog's Leg. (Hertz and Ritter.)	Explosive Gases.	Bolometer. (Rubens and Ritter.)	{ Telephone ; Air-gap } and Arc. (Lodge.)	Suspended Wires. (Hertz and Boys.)	Impulsion Cell. (Munchin.)
	Photoelectric Cell.	Expanding Wire. (Gröegory)	Vacuum Tube. (Dragounis.)		Filings. (Bradley.)
		Thermal Junction. (Klemencich.)	Galvanometer. (Kitzgerald)		Coherer. (Hughes and Lodge.)
			Air-gap and Electroscope (Beitzmann.)		
			Trigger Tube. (Warburg and Zehender.)		

× The cross against the frog's leg indicates that it does not appear really to respond to radiation unless stimulated in some secondary manner. The names against the other things are unimportant, but suggest the persons who applied the detector to electric radiation. The interrogation mark against Selenium indicates that its position in the microphonic column may be doubtful.

A handy portable exciter of electric waves is one of the ordinary hand electric gas-lighters, containing a small revolving doubler, i. e. an inductive or replenishing machine. A coherer can feel a gas-lighter across a lecture theatre. Minchin often used them for stimulating his impulsion cells. I find that when held near they act a little even when no ordinary spark occurs, plainly because of the little incipient sparks at the brushes or tinfoil contacts inside. A Voss machine acts similarly, giving a small deflection while working up before it sparks.

And notice here that our model eye has a well-defined range of vision. It cannot see waves too long for it. The powerful disturbance caused by the violent flashes of a Wimshurst or Voss machine it is blind to. If the knobs of the machine are well polished it will respond to some high harmonics, due to vibrations in the terminal rods; and these are the vibrations to which it responds when excited simply by an induction coil. The coil should have knobs instead of points. Sparks from points or dirty knobs hardly excite the coherer at all. But hold a well-polished sphere or third knob between even the dirty knobs of a Voss machine, and the coherer responds at once to the surgings got up in it.

Feeble short sparks, again, are often more powerful exciters than are strong long ones. I suppose because they are more sudden.

This is instructively shown with an electrophorus lid. Spark it to a knuckle, and it does very little. Spark it to a knob and it works well. But now try this experiment:—first spark it to an insulated sphere, there is some effect; discharge the sphere, and take a second spark, without recharging the lid; do this several times; and at last, when the spark is inaudible, invisible, and otherwise imperceptible, the coherer some yards away responds more violently than ever, and the spot of light rushes from the scale.

If a coherer be attached by a side wire to the gas pipes, and an electrophorus spark be given to either the gas pipes or the water pipes, or even to the hot-water system in any other room of the building, the coherer responds.

In fact, when thus connected to gas-pipes one day when I tried it, the spot of light could hardly keep still for five seconds. Whether there was a distant thunderstorm, or whether it was only picking up telegraphic jerks, I do not know. The jerk of turning on or off an extra Swan lamp can affect it when sensitive. I hope to try for long-wave radiation from the sun, filtering out the ordinary well known waves by a blackboard or other sufficiently opaque substance.

We can easily see the detector respond to a distant source of radiation now, viz. to a 5-inch sphere placed in the library between secondary coil knobs; separated from the receiver, therefore, by several walls and some heavily gilded paper, as well as by 20 or 30 yards of space (Fig. 19).

Also I exhibit a small complete detector made by my assistant, Mr. Davies, which is quite portable and easily set up. The essentials (battery, galvanometer and coherer) are all in a copper cylinder three inches by two. A bit of wire a few inches long, pegged into it, helps it to collect waves. It is just conceivable that at some distant date, say by dint of inserting gold wires or powder in the retina, we may be enabled to see waves which at present we are blind to.

Observe how simple the production and detection of Hertz waves are now. An electrophorus or a frictional machine serves to excite them; a voltaic cell, a rough galvanometer, and a bad contact serve to detect them. Indeed, they might have been observed at the beginning of the century, before galvanometers were known. A frog's leg or an iodide of starch paper would do almost as well.

A bad contact was at one time regarded as a simple nuisance, because of the singularly uncertain and capricious character of the current transmitted by it. Hughes observed its sensitiveness to sound waves, and it became the microphone. Now it turns out to be sensitive to electric waves, if it be made of any oxidisable metal (not of carbon),\* and we have an instrument which might be called a micro-something, but which, as it appears to act by cohesion, I call at present a coherer. Perhaps some of the capriciousness of an anathematised bad contact was sometimes due to the fact that it was responding to stray electric radiation.

The breaking down of cohesion by mechanical tremor is an ancient process, observed on a large scale by engineers in railway axles and girders; indeed, the cutting of small girders by persistent blows of hammer and chisel reminded me the other day of the tapping back of our cohering surfaces after they have been exposed to the welding effect of an electric jerk.

If a coherer is shut up in a complete metallic enclosure, waves cannot get at it, but if wires are led from it to an outside ordinary galvanometer, it remains nearly as sensitive as it was before (nearly, not quite), for the circuit picks up the waves and they run along the insulated wires into the closed box. To screen it effectively, it is necessary to enclose battery and galvanometer and every bit of wire connection; the only thing that may be left outside is the needle of the galvanometer. Accordingly, here we have a compact arrangement of battery and coil and coherer, all shut up in a copper box (see left-hand side of Fig. 21). The coil is fixed against the side of the box at such height that it can act conveniently on an outside

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\* Fitzgerald tells me that he has succeeded with carbon also. My experience is that the less oxidisable the metal, the more sensitive and also the more troublesome is the detector. Mr. Robinson has now made me a hydrogen vacuum tube of brass filings, which beats the coherer for sensitiveness. July, 1894.

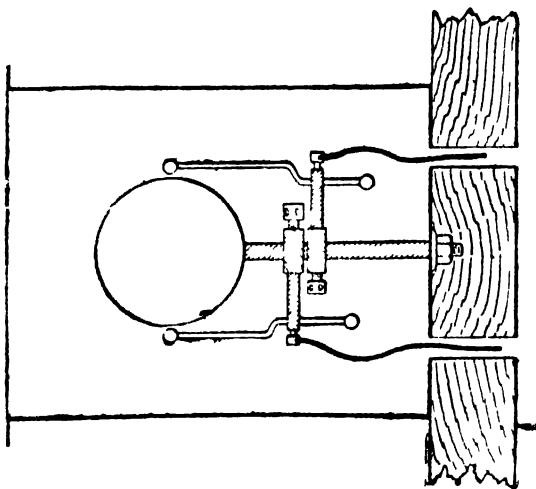
I wish to express my obligation to Mr. Edward E. Robinson for his extremely competent aid with all these experiments.

suspended compass needle. The slow action of the coil has no difficulty in getting through copper, as every one knows; only a perfect conductor could screen off that, but the Hertz waves are effectively kept out by sheet copper.

It must be said, however, that the box must be exceedingly well closed for the screening to be perfect. The very narrowest chink permits their entrance, and at one time I thought I should have to solder a lid on before they could be kept out entirely. Clamping a copper lid on to a flange in six places was not enough. But by the use of pads of tinfoil, chinks can be avoided, and the inside of the box becomes then electrically dark.

If even an inch of the circuit protrudes, it at once becomes

FIG. 20.



Spherical Radiator for emitting a Horizontal Beam, arranged inside a Copper Hat, fixed against the outside of a metal-lined box One-eighth natural size. The wires pass to the coil and battery inside the box through glass tubes not shown.

slightly sensitive again; and if a mere single wire protrudes through the box, provided it is insulated where it passes through, the waves will utilise it as a speaking tube, and run blithely in. And this whether the wire be connected to anything inside or not, though it acts more strongly when connected.

In careful experiments, where the galvanometer is protected in one copper box and the coherer in another, the wires connecting the two must be encased in a metal tube, Fig. 21, and this tube must be well connected with the metal of both enclosures, if nothing is to get in but what is wanted.

Similarly, when definite radiation is desired, it is well to put the radiator in a copper hat, open in only one direction (Fig. 20). And in



order to guard against reflected and collateral surgings running along the wires which pass outside to the exciting coil and battery, as they are liable to do, I am accustomed to put all these things in a packing case lined with tinfoil, to the outside of which the sending hat is fixed, and to pull the key of the primary exciting circuit by a string from outside.

Even then, with the lid of the hat well clamped on, something gets out, but it is not enough to cause serious disturbance of qualitative results. The sender must evidently be thought of as emitting a momentary blaze of light which escapes through every chink. Or, indeed, since the waves are some inches long, the difficulty of keeping them out of an enclosure may be likened to the difficulty of excluding sound; though the difficulty is not quite so great as that, since a reasonable thickness of metal is really opaque. I fancied once or twice I detected a trace of transparency in such metal sheets as ordinary tinplate, but unnoticed chinks elsewhere may have deceived me. [Further investigation fails to detect real transparency even in good tinfoil.]

One thing in this connection is noticeable, and that is how little radiation gets either in or out of a small round hole. A narrow long chink in the receiver box lets in a lot; a round hole the size of a shilling lets in hardly any, unless indeed a bit of insulated wire protrudes through it like a collecting ear trumpet.

It may be asked how the waves get out of the metal tube of an electric gas-lighter. But they do not; they get out through the handle, which being of ebonite is transparent. Wrap up the handle tightly in tinfoil, and a gas-lighter is powerless.

#### OPTICAL EXPERIMENTS.

And now in conclusion I will show some of the ordinary optical experiments with Hertz waves, using as source either one of two devices; either a 5-inch sphere with sparks to ends of a diameter, Fig. 19, an arrangement which emits 7-inch waves but of so dead-beat a character that it is wise to enclose it in a copper hat to prolong them and send them out in the desired direction; or else a 2-inch hollow cylinder with spark knobs at ends of an internal diameter, Fig. 12. This last emits 3-inch waves of a very fairly persistent character, but with nothing like the intensity of one of the outside radiators.

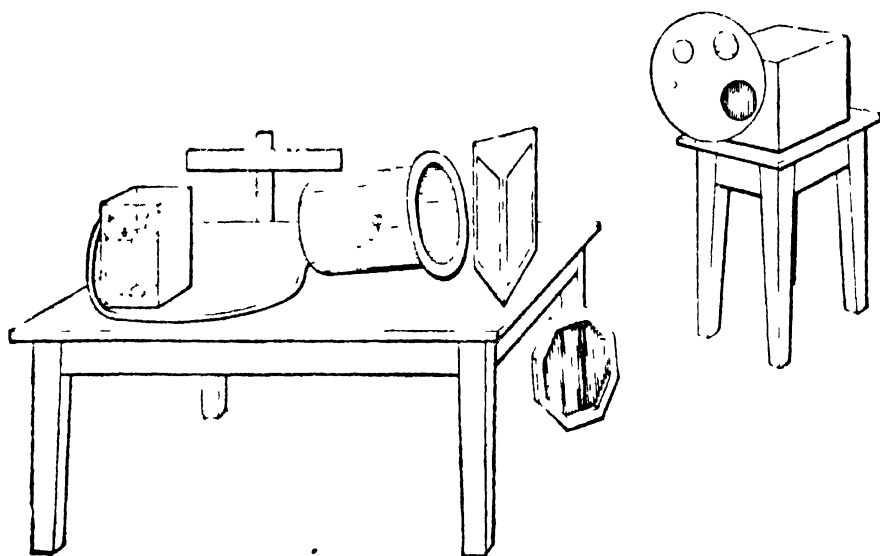
As receiver there is no need to use anything sensitive, so I employ a glass tube full of coarse iron filings, put at the back of a copper hat with its mouth turned well askew to the source, which is put outside the door at a distance of some yards, so that only a little direct radiation can reach the tube. Sometimes the tube is put lengthways in the hat instead of crossways, which makes it less sensitive, and has also the advantage of doing away with the polarising, or rather analysing, power of a crossway tube.

The radiation from the sphere is still too strong, but it can be stopped down by a diaphragm plate with holes in it of varying size, clamped on the sending box, Fig. 21.

Having thus reduced the excursion of the spot of light to a foot or so, a metal plate is held as reflector, and at once the spot travels a couple of yards. A wet cloth reflects something, but a thin glass plate, if dry, reflects next to nothing, being, as is well known, too thin to give anything but "the black spot." I have fancied that it reflects something of the 3-in. waves.

With reference to the reflecting power of different substances it

FIG. 21.



General arrangement for optical experiments; showing Metal Box on a Stool, inside which the Radiators were fixed (this box was really like Fig. 20); the Copper Hat containing the Coherer, with the metal Box containing Battery and Galvanometer Coil connected to it by a compo pipe conveying the wires; a Paraffin Prism; and a Polarising Grid.

may be interesting to give the following numbers, showing the motion of the spot of light when 8-in. waves were reflected into the copper hat, the angle of incidence being about 45°, by the following mirrors:—

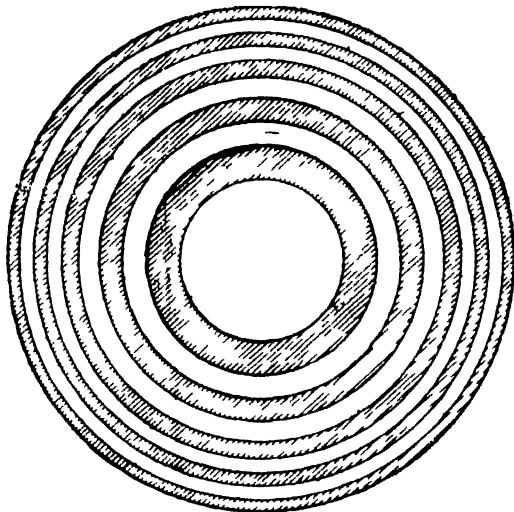
Sheet of window glass	.. .. .	0 or at most 1 division.
Human body	.. .. .	7 divisions.
Drawing board	.. .. .	12 "
Towel soaked with tap-water	.. .. .	12 "
Tea-paper (lead?)	.. .. .	40 "
Dutch metal paper	.. .. .	70 "
Tinfoil	.. .. .	80 "
Sheet copper	.. .. .	100 and up against steps.

A block of paraffin about a cubic foot in volume is cast into the shape of a prism with angles  $75^\circ$ ,  $60^\circ$ , and  $45^\circ$ . Using the large angle, the rays are refracted into the receiving hat, Fig. 21, and produce an effect much larger than when the prism is removed.

An ordinary 9-in. glass lens is next placed near the source, and by means of the light of a taper it is focussed between source and receiver. The lens is seen to increase the effect by concentrating the electric radiation.

The lens helps us to set correctly an 18-in. circular copper disc in position for showing the bright diffraction spot. Removing the disc the effect is much the same as when it was present; in accordance with the theory of Poisson. Add the lens and the effect is greater.

FIG. 22.



Zone-plate of Tinfoil on Glass  
Every circular strip is of area equal to central space.

With a diffraction grating of copper strips 2 in. broad and 2 in. apart, I have not yet succeeded in getting good results. It is difficult to get sharp nodes and interference effects with these sensitive detectors in a room. I expect to do better when I can try out of doors away from so many reflecting surfaces; indoors it is like trying delicate optical experiments in a small whitewashed chamber well supplied with looking-glasses; nor have I ever succeeded in getting clear concentration with this zone-plate having Newton's rings fixed to it in tinfoil. But really there is nothing of much interest now in diffraction effects except the demonstration of the waves and the measure of their length. There was immense interest in Hertz's time, because then the wave character of the radiation had to be proved;

but every possible kind of wave must give interference and diffraction effects, and their theory is, so to say, worked out. More interest attaches to polarisation, double refraction and dispersion experiments.

Polarisation experiments are easy enough. Radiation from a sphere is already strongly polarised, and the tube acts as a partial analyser, responding much more vigorously when its length is parallel to the line of sparks than when they are crossed; but a convenient extra polariser is a grid of wires something like what was used by Hertz, only on a much smaller scale; say an 18-in. octagonal frame of copper strip with a harp of parallel copper wires (see Fig. 21, on floor). The spark-line of the radiator being set at  $45^\circ$ , a vertical grid placed over receiver reduces the deflection to about one-half, and a crossed grid over the source reduces it to nearly nothing.

Rotating either grid a little, rapidly increases the effect, which becomes a maximum when they are parallel. The interposition of a third grid, with its wires at  $45^\circ$  between two crossed grids, restores some of the obliterated effect.

Radiation reflected from a grid is strongly polarised, of course, in a plane normal to that of the radiation which gets through it. They are thus analogous in their effect to Nicols, or to a pile of plates.

The electric vibrations which get through these grids are at right angles to the wires. Vibrations parallel to the wires are reflected or absorbed.

To demonstrate that the so-called plane of polarisation of the radiation transmitted by a grid is at right angles to the electric vibration,\* i. e. that when light is reflected from the boundary of a transparent substance at the polarising angle the electric vibrations of the reflected beam are perpendicular to the plane of reflection, I use the same paraffin prism as before; but this time I use its largest face as a reflector, and set it at something near the polarising angle. When the line of wires of the grid over the mouth of the emitter is parallel to the plane of incidence, in which case the electric vibrations are perpendicular to the plane of incidence, plenty of radiation is reflected by the paraffin face. Turning the grid so that the electric vibrations are in the plane of incidence, we find that the paraffin surface set at the proper angle is able to reflect hardly anything. In other words, the vibrations contemplated by Fresnel are the electric vibrations; those dealt with by McCullagh are the magnetic ones.

Thus are some of the surmises of genius verified and made obvious to the wayfaring man.

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\* Cf. Trouton, in 'Nature,' vol. xxxix. p. 393; and many optical experiments by Mr. Trouton, vol. xl. p. 398. Also by Klemencik, (Wied. Ann. vol. xlv.), Righi (Acc. dei Lincei, vol. xi.), and Elster and Geitel (Phil. Mag. July 1894, p. 158).

## LIST OF HERTZ'S PAPERS.

1878-79. Wied. Ann. 1880, x. p. 414. Experiments to establish an Upper Limit for the Kinetic Energy of Electric Flow.

1880. Inaugural Dissertation (Doctor Thesis) on Induction in Rotating Spheres.

1881. Wied. Ann. xiii. p. 266. On the Distribution of Electricity on the Surface of Moving Conductors.

1881. Crelle, xcii. p. 156. On the Contact of Solid Elastic Bodies.

1881. Wied. Ann. xiv. p. 581. Upper Limits for the Kinetic Energy of Moving Electricity.

1882. Verhandlungen des Vereins des Gewerbflusses (Sonderabdruck). On the Contact of Solid Elastic Bodies and on Hardness.

1882. Wied. Ann. xvii. p. 177. On the Evaporation of Liquids, especially of Quicksilver, in Air-Free Space, and on the Pressure of Mercury Vapour.

1882. Verhandln. d. phys. Gesellschaft in Berlin, p. 18. On a New Hygrometer.

1883. March. Schlömilch Zeitschrift, p. 125. On the Distribution of Pressures in an Elastic Circular Cylinder.

1883. Wied. Ann. xix. p. 78. On an appearance accompanying Electric Discharge.

1883. *ib.* xix. p. 782. Experiments on Glow Discharge.

1883. Wied. Ann. xx. p. 279. On the Property of Benzine as an Insulator and as showing Elastic Reaction (Rückstandsbildner).

1883. Zeitschrift für Instrumentenkunde. Dynamometric Contrivance of Small Resistance and Infinitesimal Self-Induction.

1884. Met. Zeitschrift, November-December. Graphic Methods for the Determination of the Adiabatic Changes of Condition of Moist Air.

1884. Wied. Ann. xxii. p. 449. On the Equilibrium of Floating Elastic Plates.

1884. *Ib.* xxiii. On the Connection between Maxwell's Electro-dynamic Fundamental Equations and those of Opposition Electro-dynamics.

1885. *Ib.* xxiv. p. 114. On the Dimension of a Magnetic Pole in different Systems of Units.

1887-1889. Papers incorporated in his book, 'Ausbreitung der Elektrischen Kraft,' translated under the title of 'Electric Waves.'

1892. Wied. Ann. xlv. p. 28. On the Passage of Cathode Rays through Thin Metal Sheets.

1894. A Posthumous work on the Principles of Mechanics exhibited on a new plan. With a Preface by Von Helmholtz.

[O. J. L.]

Friday, June 8, 1894.

SIR FREDERICK ABEL, Bart. K.C.B. D.C.L. LL.D. F.R.S.  
Vice-President, in the Chair. •

C. VERNON BOYS, Esq. F.R.S. A.R.S.M. M.R.I.

*The Newtonian Constant of Gravitation.*

It is probably within the knowledge of most of those present that Sir Isaac Newton, by his great discovery of gravitation and its laws, was able to show that a single principle, ideally simple, viz. that every particle in the universe attracts any other particle towards itself with a force which is proportional to the product of their masses divided by the square of the distance between them, would completely and absolutely account for the three laws of planetary motion which Kepler had given to the world.

Newton also showed that a spherical body, whether uniformly dense or varying in density according to any law from the centre to the surface, would attract bodies outside with the same force that it would do if it could all be concentrated at its centre, i.e. that all the attractions varying in amount and direction produced by particles in all parts of a sphere need not be considered separately, but may be treated in this simple way.\*

Nevertheless, though Newton's great discovery is sufficient to bring the whole of the movements of the planets and of their satellites, whether their simple Keplerian motions or the disturbances produced by their mutual gravitation, the motions of comets, of binary stars, of the tides, or the falling apple, under the domain of a single and simple principle, though it enables one to compare the masses of the sun, the planets and their satellites, and of those binary stars whose parallax has been determined, one thing can never be made known by astronomical research alone, though we may know that twenty-eight suns would be required to make one Sirius; that the sun is equal to 1048 Jupiters, that Jupiter is more than double all the rest of the solar system put together, or that the moon is  $1/80$  of the earth; no observations of these bodies can ever tell us how many tons of matter go to make up any one of them.

Though we know from first principles of dynamics, by the mere

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\* Only last night I learned that it was the difficulty of proving this, and not the erroneous value of the moon's distance, that delayed the publication of Newton's discovery for so long.

consideration of centrifugal force, that the whole sun attracts each ton of the earth with a force equal to a weight here of a little more than one pound, and that if it were not for this, every ton of the earth would continue its journey into space in a straight line for ever, and though we know in the same way that the whole earth attracts each ton in the moon with a force equal to the weight of ten ounces and no more, we cannot tell by any astronomical observation whatever how many tons there are in all.

Newton showed that to complete his law and to put in the numerical constant (the Newtonian Constant of Gravitation) that would convert his proportion into an equality, two methods are available: we may either make observations on the disturbance of the earth's gravitation by the action of isolated parts of it, we may either find the relative attraction of an isolated mountain or the strata above the bottom of a deep mine, or we may make an artificial planet of our own and find the attraction which it exerts.

The Newtonian Constant will be known if we know the force of attraction between two bodies which we can completely measure and weigh. Employing the C. G. S. system of measurement, the Newtonian Constant is equal to the force of attraction in dynes between two balls weighing a gramme each, with their centres one centimeter apart. Of course it may be referred to pounds and inches or tons and yards, but as soon as all the quantities but  $G$  in Newton's equation

$$\text{Force} = G \frac{\text{Mass} \times \text{Mass}}{\text{Distance}^2}$$

are known, no matter in what units the quantities are measured,  $G$  is known. The conversion of its numerical value from one system of measurement to another is of course a mere matter of arithmetic.

Of the first method of finding  $G$ , depending on the attraction of a mountain, first attempted by Bouguer at the risk of his life in the hurricanes of snow on Chimborazo, of the experiments of Maskelyne, of Airy and of others, I cannot now find time to speak; I can only refer to Poynting's essay on the subject. It is the second method with an artificial planet that I have to describe to-night.

Now let me give some idea of the minuteness of the effect that has to be measured. Is a wall built true by the aid of a plumb-line vertical, or does it lean outwards? Newton's principle shows that the plumb-bob is attracted by the wall, yet it hangs vertically. The attraction is so small that it cannot be detected in this way. Even the attraction of a whole mountain requires the most refined apparatus to make its existence certain. Do two marbles lying on a level table rush together? According to Newton's principle they attract one another; yet if they were a thousand times smoother than they are, no movement of attraction could be detected.

Leaving matters of common experience, let us go into the physical laboratory where instruments of the highest degree of precision and

delicacy (at least so they are called) are found on every table. What precautions are taken to prevent the attractions of the fixed and moving parts from interfering with the result which they are constructed to measure? None. The attractions are so small, that in no apparatus in use for the measurement of electrical, magnetic, thermal or other constants are they ever thought of, or is any provision necessary to prevent their falsifying the result. Nevertheless, the attractions exist, and if only the means are delicate enough they can be detected and measured. The Rev. John Mitchell was the first to devise a successful method. He was the first to invent the torsion balance with which Coulomb made his famous electrical researches, and which bears Coulomb's name. He devised and he made apparatus for this purpose, but he did not live to make any experiments.

After his death Cavendish remodelled Mitchell's apparatus and performed the famous Cavendish experiment. By means of the apparatus, of which for the second time I show a full-size model in this theatre, Cavendish measured the force of attraction between two balls of lead, one 12 and the other 2 inches in diameter, and with their centres 8.85 inches apart. The same experiment has since been made by Reitch, by Baily, and more recently by Cornu and Baille with greatly superior apparatus of one quarter of the size. All these observers actually determined the attraction between masses which could be weighed and measured, and thus found with different degrees of accuracy the value of  $G$ .

Let me explain now that this  $G$ , the gravitation constant, or as I prefer to call it, for the sake of distinction, the Newtonian Constant of Gravitation, has nothing to do with that other quantity generally written  $g$ , which represents the attraction at the earth's surface. This is a purely accidental quantity, which depends not only upon  $G$ , but also upon the size of the earth, its mean density, the latitude, the height above the sea, and finally upon the configuration and the composition of the neighbouring districts.  $g$  is eminently of a practical and useful character; it is the delight of the engineer and the practical man; it is not the same in all places, but that he does not mind. It is of the earth, arbitrary, incidental and vexatious. Prof. Greenhill should spell his name with a little  $g$ .  $G$ , on the other hand, represents that mighty principle under the influence of which every star, planet and satellite in the universe pursues its allotted course; it may possibly also be the mainspring of chemical action. Unlike any other known physical influence, it is independent of medium, it knows no refraction, it cannot cast a shadow. It is a mysterious power, which no man can explain; of its propagation through space, all men are ignorant. It is in no way dependent on the accidental size or shape of the earth; if the solar system ceased to exist it would remain unchanged. I cannot contemplate this mystery, at which we ignorantly wonder, without thinking of the altar on Mars' hill. When will a St. Paul arise able to declare it unto us? Or is gravitation, like life, a mystery that can never be solved?



Owing to the universal character of the constant  $G$ , it seems to me to be descending from the sublime to the ridiculous to describe the object of this experiment as finding the mass of the earth, or the mean density of the earth, or, less accurately, the weight of the earth. I could not lecture here under the title that has always been chosen in connection with this investigation. In spite of the courteously expressed desire of your distinguished and energetic secretary, that I should indicate in the title that, to put it vulgarly, I had been weighing the earth, I could not introduce as the object of my work anything so casual as an accidental property of an insignificant planet. To the physicist this would be equivalent to leaving some great international conference to attend to the affairs of a parish council. That is the business of the geologist. The object of this investigation is to find the value of  $G$ . The earth has no more to do with it than the table has upon which the apparatus is supported. It does interfere, and occasionally, by its attraction, breaks even the quartz fibres that I have used. The investigation could be carried on far more precisely and accurately on the moon, or on a minor planet, such as Juno; but as yet no means are available for getting there.

I shall not have time to-night to describe the work of former investigators, and for this there is little need, since it is all collected in Poynting's Adams prize essay, "On the Mean Density of the Earth," published this year. I cannot even find time to explain in more than the merest outline what I have done to develop the apparatus of Cavendish, so that he would hardly recognise in my glorified bottle-jack the balls and lever which have made his name famous. The following table, given by Poynting, however, represents the results of the labours of investigators up to the present time.

In connection with this table I cannot lose the opportunity of quoting Newton's extraordinary prophecy, marvellous in that without any direct knowledge he gave a figure which was nearer the truth than that found by many of the experimenters that came after him. The passage is as follows:—

"*Undo cum Terra communis suprema quasi duplo gravior sit quam aqua, et paulo inferius in fodinis quasi triplo vel quadruplo aut etiam quintuplo gravior reperiatur; verisimile est quod copia materiæ totius in Terra quasi quintuplo vel sextuplo major sit quam si tota ex aqua constaret; præsertim cum terram quasi quintuplo densiorem esse quam Jovem jam ante ostensum sit.*" \*

I have placed on the wall the diagram of the apparatus which I showed in action when lecturing here upon quartz fibres five years ago. With this I was able, for the first time, to show to an audience the effect of the very small attraction exerted between a 2-inch cylinder of lead and a little one weighing only a gramme or 15 grains. The apparatus which I have to describe to-night is the same in principle, the main distinction being that it is so designed and

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\* Newton's 'Principia,' second edition, 1714, p. 373, line 10.

## SUMMARY OF RESULTS HITHERTO OBTAINED.

Approximate date.	Experimenter.	Method.	Result.
1737-40	Bouguer .. ..	Plumb-line and pendulum.	$\Delta$ Inconclusive
1774-76	Maskelyne and Hutton	Plumb-line .. ..	4.5-5
1855	James and Clarke ..	" .. ..	5.316
1821	Carlhi .. ..	Mountain pendulum ..	4.39-4.95
1880	Mendenhall .. ..	" .. ..	5.77
1854	Airy .. ..	Mine pendulum .. ..	6.565
1883	Von Sterneck .. ..	" .. ..	5.77
1885	Von Sterneck .. ..	" .. ..	About 7
1797-98	Cavendish .. ..	Torsion balance .. ..	5.148
1837	Reich .. ..	" .. ..	5.49
1810-41	Baily .. ..	" .. ..	5.674
1872	Reich .. ..	" .. ..	5.583
1870	Cornu and Baille ..	" .. ..	5.56-5.50
1849	Boys .. ..	" .. ..	In progress
1879-80	Von Jolly .. ..	Common balance .. ..	5.692
1878-90	Poynting .. ..	" .. ..	5.493
1884	König, Richarz, and Krüger Menzel.	" .. ..	(5.16-5.52) In progress
1886-88	Wilsing .. ..	Pendulum balance .. ..	5.579
1889	Laska .. ..	" .. ..	In progress

constructed that I can tell precisely where every gravitating particle is placed. In the design of this apparatus I have been, as every one will admit, bold—most would have preferred the word reckless; but knowing the truth of the principles which I had developed, and having faith and confidence in the quartz fibre, I deliberately chose to reduce all the dimensions to an extent which caused the forces, and especially the couples, to be insignificant in comparison with any which had been within the reach of the experimenter hitherto. The whole difficulty of Cavendish, Reich and Baily had been to measure so minute an effect; instead of increasing this I diminished it enormously, being satisfied that I should be able to make a proportionately more accurate measure by so doing. Cornu reduced the dimensions to one-quarter; I have reduced the chief one to one-eightieth. Cavendish had a force equal to  $1/3650$  grain's weight to measure; I have less than a five-millionth. By the use of the long lever, Cavendish had the effect of a force of  $1/100$  grain's weight on an arm an inch long; I have less than a twelve-millionth of a grain on an arm of that length. His forces were fourteen hundred times as great as mine; his couples or twisting forces were a hundred and twenty thousand times as great. One advantage gained by the use of small apparatus, in which alone the attracting balls can be made large compared with the length of the beam, is the increased

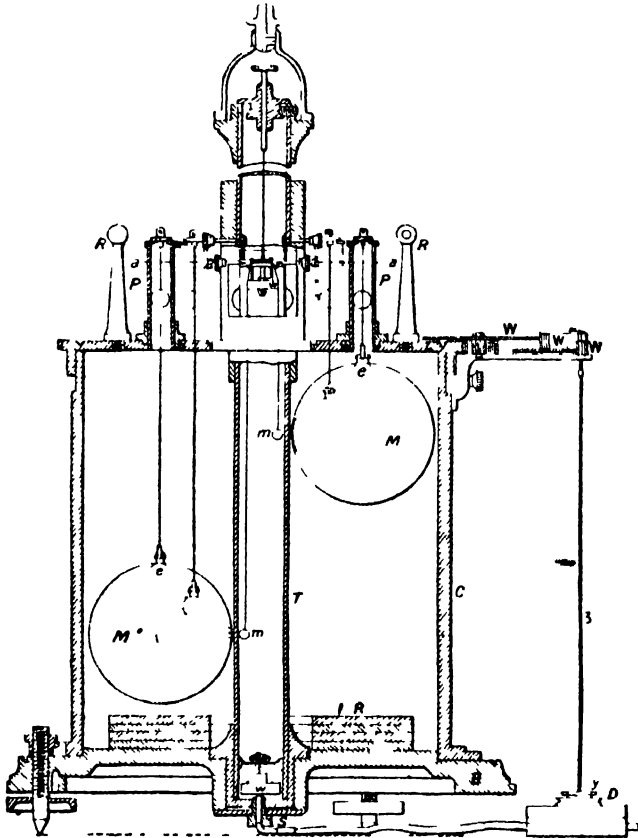
sensibility, the greater angle of deflection produced by the attractions when the period of oscillation is the same. This is more especially the case in my apparatus where the two sides are at different levels. But the question arises whether this reduction of linear dimensions may not introduce irregularity and want of stability to such an extent as to more than counterbalance the advantages to which I have already referred. In spite of every endeavour that may be made to keep the air quiet, to exclude draughts, to keep all the apparatus at one temperature in a vault of constant temperature, infinitesimal differences must exist; one side of the apparatus must be hotter than the other, though no thermometer could be made which would detect the difference. In consequence of this difference of temperature the air circulates, and so creates a draught which blows upon the mirror and the suspended balls. I had erroneously come to the conclusion that in apparatus geometrically similar these disturbances would probably be in the proportion of the seventh power of the linear dimensions, so that greatly increased stability would be obtained by reducing the dimensions. However, I have discussed this matter with Prof. Poynting, who has shown me that in reality the disturbances would be proportional to the fifth power of the linear dimensions if, as should be the case, the circulation of the air were so extremely slow that the motion would be steady, whereas it would gradually rise from this to the eighth power as the term involving the square of the velocity increased in magnitude and the motion became irregular. So long, therefore, as the apparatus is small enough to prevent terms involving the square of the velocity from being appreciable, the stability is the same whatever the size, but as soon as the apparatus exceeds this, then the disadvantage of size very rapidly becomes evident. In addition to this, the time needed to bring the apparatus to a steady state is far greater with large apparatus. After making the geometrical measures I leave my apparatus, small as it is, three days, if possible, before observing deflections and periods.

The diagram, Fig. 1, is a vertical section through the apparatus. B and C represent an accurately turned brass box with a lid L, which can be made to turn round insensibly by the action of the wheels W W. The lid carries two tubular pillars P P, from the tops of which the balls M M hang by phosphor bronze wires, being definitely held in place by geometrical clamps on the heads of the pillars. The lid also carries two supporting pillars R R. In the centre tube the "beam mirror" N hangs by means of a quartz fibre from an adjustable torsion head surmounted by a bell jar, and from the ends of the mirror the two gold balls *m m* hang by separate quartz fibres. Four rings of india-rubber are placed on the base to prevent destruction of the apparatus in case the balls should drop by any accident. Now it is evident that if the lid is turned from the position in which it is shown, that is, with all four balls in one plane, in which position the attractions do not tend to twist the central torsion fibre at all, then these attractions will produce a

couple increasing with the angle up to a certain point ( $65^\circ$  in the particular case), after which the couple falls off again and becomes zero when it has turned  $180^\circ$ .

Since the effect is a maximum at  $65^\circ$ , very great accuracy in the measurement of this angle is of little consequence. By means of a small telescope at a distant table, and the divided edge and vernier, I

FIG. 1.



By permission of the *Engineer*.

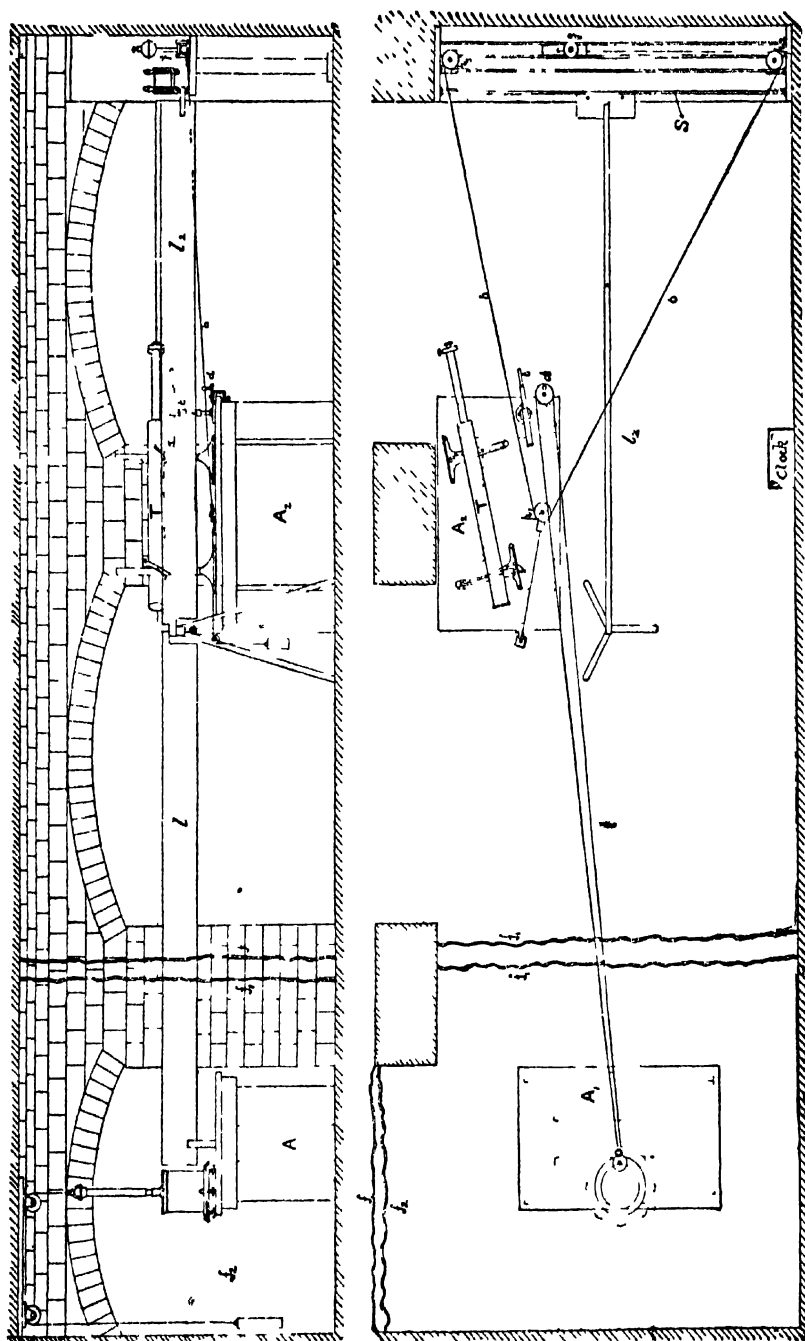
2-2 y

can tell the angle with certainty to  $1/20$  degree; an uncertainty of one-quarter of a degree would be of but little consequence. Again, if the pair of gold balls twist about an axis which is not exactly that round which the lead balls are carried, if there is any small eccentricity of the gold or lead balls, then eccentricity in the common plane removes the gold balls from a position of minimum effect, eccentricity across the plane removes them from a position of

maximum effect, and if the levels of the gold balls are not precisely the same as those of the lead balls, again the departure is from a position of maximum effect. All these three eccentricities can be determined with an accuracy of  $1/1000$  inch. Errors of  $1/100$  inch would make a barely perceptible effect upon the result. The design, therefore, is such that a great number of measures which are difficult, and can at the best only be made with a second quality degree of accuracy, are of so little consequence that this degree is more than abundant. The final result depends directly upon a few measures which, as I hope to show, can be made with facility and most accurately. These are the horizontal distance from centre to centre of the wires by which the lead balls are suspended, the horizontal distance between the centres of the quartz fibres by which the gold balls are suspended, the angle through which the mirror is deflected, the masses of the lead but not of the gold balls, and the natural time of oscillation of the mirror when the balls are suspended and when a thin cylinder of small moment of inertia, but of the same weight as the balls, is suspended axially in their stead.

Before going more into detail and showing how the operations are carried out so that all the quantities may be known with a sufficient degree of accuracy, it will be convenient to project upon the screen a drawing of the vault in which the experiments have been made. Prof. Clifton has kindly allowed me the free use of the vault under the Clarendon Laboratory at Oxford. This is shown in Fig. 2, of which the upper portion represents an elevation and the lower part a plan. The instrument itself stands upon the table  $A_1$  in the corner, where it is screened from temperature disturbances, which my presence in the distant corner and a very small flame produce, by an octagon house of double wood lined with cotton-wool, and by double felt screens  $f_1 f_2$ . On the second table  $A_2$  are placed a large astronomical telescope  $T$ , through which the large scale  $S$  is seen by reflection from the mirror in the apparatus, a small reading telescope  $t$  to read the angle of the lid and vernier, a pulley-wheel  $p_1$  and a driving-wheel  $d$ . The pulley-wheel  $p_1$  keeps the cord  $b$  which passes round  $p_2$  and  $p_3$ , and is attached to the cart  $g$ , always tightly stretched, so that the observer at the telescope can always keep a little flame carried by the cart immediately behind the particular division under observation. The driving wheel  $d$  is made with a very large moment of inertia, and the handle is near the axis, so that its motion is necessarily steady. A very light cord passes round this, across the room, and after passing through a hole in the screen passes also round the little wheel  $D$ , Fig. 1, and thus serves to drive the train  $W W$ , and so carry the lid and balls round almost insensibly. Two hundred and thirty turns of  $d$  are required to move the lead balls from the  $+$  to the  $-$  position. I generally turn the handle a hundred and thirty times, and then when the mirror is approaching an elongation, turn the handle the remaining hundred times, finally stopping when the lid reading, as observed in the small

FIG. 2.



telescope, is correct. The large scale S is 9 feet long, and is divided into fiftieths of an inch. There are 4800 divisions.

Two beams  $l_1$   $l_2$  are seen in Fig. 2. The upper surfaces of these are straight, and are adjusted by screws until they are truly level. These are used when the true optical distance from the mirror to the scale is being measured. A steel tape, on which I engraved a fine line near each end, rests upon the beams. At one end a slider carrying a microscope is placed so as to see a fine line at the centre of the mirror accurately in focus, while at the other a corresponding slider is placed so that a projecting brass rod rests against the scale. At the same time cross lines engraved upon the plate-glass bases are placed exactly over the lines engraved on the steel tape. When afterwards the microscope is focussed upon the end of the brass rod, the distance between the cross lines, as measured by a scale, is the amount that has to be added to the distance between the engraved lines upon the tape, in order to obtain the distance from the scale to the mirror.

Overhead wheels are shown in Fig. 2, fastened to the roof above the apparatus, and again close to the end wall. These serve many purposes, as will appear later. Among others, the middle one of each carries a cord fastened at one end to a crossbar joined at its ends by guys to the pillars R of the lid, Fig. 1, and at the other to heavy balance weights to counterbalance the balls M M and part of the lid. Thus the friction is greatly reduced, and the tremor set up by rotating the lid is in a corresponding degree slight.

All time observations are made chronographically upon a drum by the Cambridge Scientific Instrument Company. This is placed in the adjoining vault. Two time-markers record with their points less than  $1/100$  inch apart, one of them marking every second of the clock with special marks for minutes and half-minutes, and the other every depression of the key at my right hand. The late Prof. Pritchard kindly lent me an astronomical clock for the purpose, to which I fitted time-marking contacts; but into the details of these I must not enter. He also allowed me to make use of one of his assistants to keep me informed of the rate of the clock from time to time.

I have up to the present spoken vaguely of the large lead balls and of the small gold balls, but have given no indication as to how they are made and how I can be sure of the truth of their form and their homogeneity. Mr. Munro, whose capacity for turning accurate spherical work is well known, made for me two moulds of hard cast iron, which I have on the table. One of these is for a  $4\frac{1}{4}$ -inch lead ball, and one for a  $2\frac{1}{4}$ -inch lead ball. Each mould is made in two halves, so truly as to shape and size that the thin steel disc that was used as a template would distinctly rattle when in its place, but when a strip of cigarette-paper was inserted on one side it could not be got in at all. The upper half of each of these moulds is provided with a cylindrical steel plunger accurately fitting a central hole in the mould, and with its end turned to the same spherical surface when it is

pressed home upon its shoulder. The lower half of each mould has a  $\frac{1}{4}$ -inch central cylindrical hole, into which the lug of the brass ball holder exactly fits. There is also a small hole at the side which can be stopped with a brass plug. The balls are made as follows:—The interior of the mould is smoked and then screwed up as tight as possible. It is then heated until a piece of lead upon it begins to melt. The necessary quantity of pure lead melted in an earthen pot is then carefully skimmed and poured in until the cylindrical neck is full. The mould is then made to rest upon a cold iron slab, and a large blowpipe flame is directed upon the upper part so that it cools from below upwards, and not from the surface inwards; more lead is added to keep the neck full. As soon as the lead in the neck solidifies the plunger is inserted, and the whole is placed in a hydraulic press. The plunger is forced down upon its seat; the lead, already free from bubbles and vacuous cavities, is compressed until at last the excess of solid metal flows through the small side hole in the form of wire. The ball is thus made true in form, necessarily homogeneous, which no alloy is likely to be, and definite in size. When cold it can be lifted from the mould, then, after cutting off the wire which projects from its equator, it is ready for weighing.

The small gold balls are made by melting the required quantity of pure gold in a hole in a bath brick, and, as in the case of the lead, letting it cool from below upwards, so as to avoid cavities. It is then inserted in a pair of polished hemispherical hardened steel dies, which Mr. Colebrook made for the purpose, and beaten, being turned between each blow, and annealed once or twice until a perfect polished sphere, without a mark upon it, is the result. I make these in pairs of exactly the same weight, and, as in the case of the lead balls, thus obtain truth of form, accuracy of size and homogeneity, all in a very perfect—more than sufficiently perfect—degree. These are each suspended from a quartz fibre of the necessary length, to the other end of which a hook and eye is fastened. Into the very important details of these operations it is impossible, for want of time, for me to enter. The gold balls are  $\cdot 2$  and  $\cdot 25$  inch in diameter, and a pair of gold cylinders were made in a similar tool  $\cdot 25$  inch in diameter, and about the same length.

Perhaps the most important detail in the whole apparatus is the "beam mirror," which is of the form shown in Fig. 3. It is necessary, as far as possible, to reconcile the following incompatible conditions. It should be as light as possible, have as small a moment of inertia as possible, the optical definition should be as perfect as possible, and, almost most important of all, the form should be such that the resistance offered by the viscosity of the air should be reduced to the smallest possible degree. By cutting the middle portion out of an optically perfect round mirror all these conditions are realised in some degree, and the optical definition is actually more perfect in the horizontal direction than that due to the whole disc. This is fastened to a cross-shaped support of gilt copper. The ends of the mirror have vertical grooves of microscopic fineness cut in their thickness, so



that the quartz fibre hanging from the cross-arm above may rest definitely in them. The central hook is for the purpose of hanging the "counterweight," i.e. a slender silver cylinder of exactly the same weight as the gold balls with their fibres and hooks. By this means the unknown moment of inertia of the mirror may be eliminated with the fibre equally stretched in both cases, a most necessary condition,

for I have found that the torsional rigidity is seriously affected by variation in stretching.

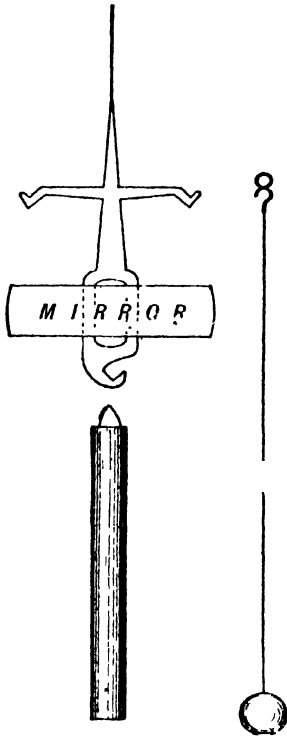
Means are provided by which I can effect the transfer of the gold balls from the beam to the side hooks or the reverse, or change their places without opening the window; but these and numerous other important details I must pass over.

Unfortunately accidents are liable to happen, and, as I know by dearly-bought experience, the gold balls may sometimes be precipitated down the central tube. I have recovered them sometimes by an india-rubber tube, let down through the window aperture, sucking at the other end until they closed the open end, when they could be drawn up. Latterly I have made use of a magnetised tuning-fork to pick up a very small fragment of iron tied to a silk line, by means of which I could draw up a diaphragm with anything that might have fallen upon it.

I have already stated that two measurements, viz. the horizontal distances between the axes of the wires which support the lead balls, and of the fibres which support the gold balls, must be made with the highest degree of accuracy attainable, for on these the result directly depends. In order to accomplish this I had to design a special instrument, an

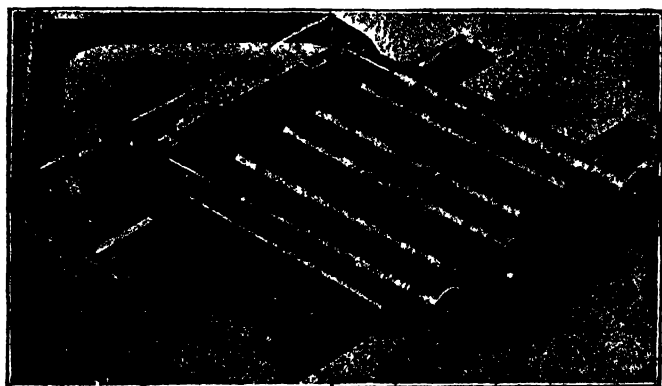
optical compass, which is illustrated in Fig. 4. This is an arrangement which rests upon the lid of the apparatus on the circular V-groove seen in Fig. 1, so that it can rotate without shake. Upon the lower framing rests the focussing slide, and on this a pair of traversing slides, each carrying a microscope in one or other of three grooves. The two traversing slides are drawn together by a spring, and can be separated by a screw cone, forming a very delicate fine adjustment. This is operated by the screw-head  $S_4$ ;  $S_3$  is a focussing screw giving a fine adjustment to the focussing slide.  $S_2, S_2$  are two parallelising screws, the purpose of which is to bring the microscopically-divided glass scale into focus at each end simulta-

FIG. 3.



neously.  $S_1$  is a micrometer screw-head, which is employed to push the scale bodily to the right by measured amounts. The two microscopes are focussed upon, say, the right sides of the wires, the focussing slide is then withdrawn, leaving them relatively unchanged, and the microscopic scale is then put in its place. The distance from wire to wire is thus transferred directly to the scale, and the fractional part of any one division of  $1/100$  inch is all that has to be referred to and measured by the screw. Every slide in this apparatus is geometrically arranged, so that the movements are all perfectly free, unconstrained and without shake. In measuring the distance between the fibres, which must be done while they are freely suspended, so that a force of a millionth of the weight of a grain will give them a considerable motion, means have to be provided to exclude draught, which yet must not interfere with the apparent distances of the fibres. No microscope cover-glass is any use for this

FIG 4.

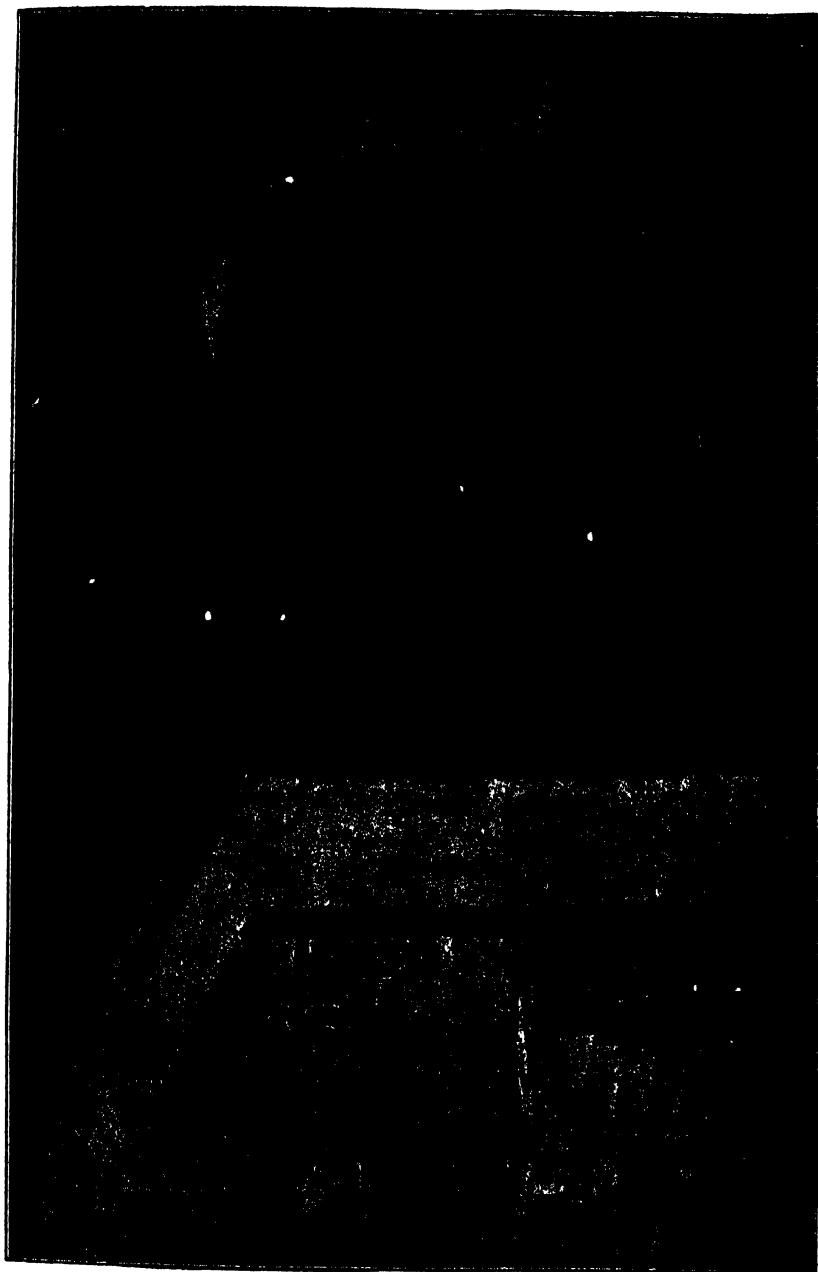


purpose. It is sure to be prismatic, and when inserted between the microscope and an object it will certainly cause it to shift its apparent position. A piece of clear mica is perfect in this respect, no movement, even with a high power, being visible. I mention this as it well illustrates the sort of trap that is ever set for the experimentalist. If I had not been aware of this, and had used, as would be natural, a window of microscope cover-glass, then each fibre would have appeared as definitely in its place as before, but the place would have been wrong, perhaps by  $1/1000$  inch, and thus a consistent error affecting all the experiments alike would have been introduced, and no multiplication of observations or taking of means would have eliminated it. It is on this account that it is so important in experimental work to vary the conditions in every way, so as to discover unsuspected consistent errors.

The microscope scale was made by Zeiss, and is a most perfect example of scale construction. In order to test the accuracy and find the errors of the scale, I took advantage of my visit to Cardiff for the

meeting of the British Association, to compare it with a series of Whitworth standard bars on Prof. Viriamu Jones's very perfect

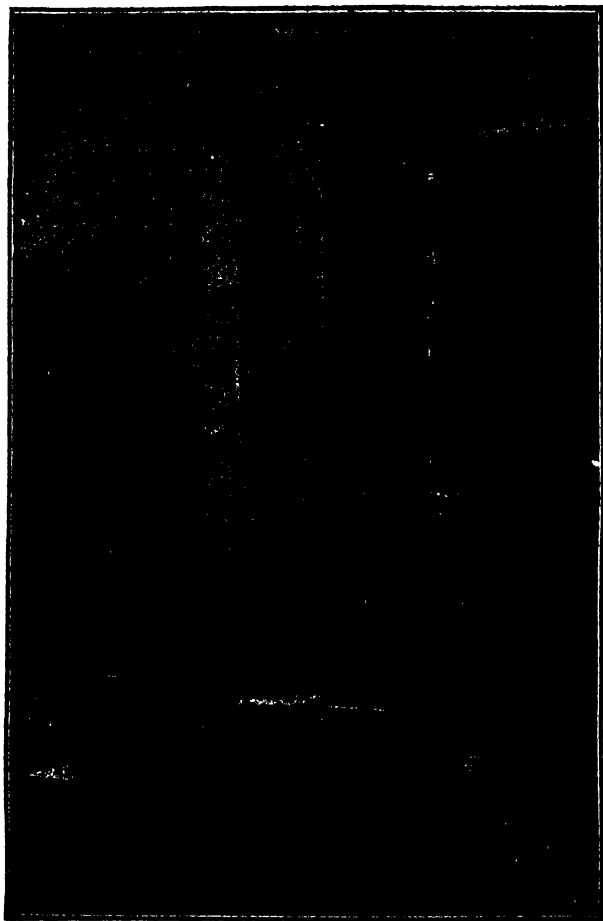
FIG. 5.



Whitworth measuring machine. For this class of work sunshine or dust give great trouble, but I was fortunate in having splendid weather for my purpose, as visitors will probably remember: it rained without ceasing during the two days I was making these measurements.

Having now very imperfectly described the apparatus and the place in which I have carried out my experiments, I will next show a

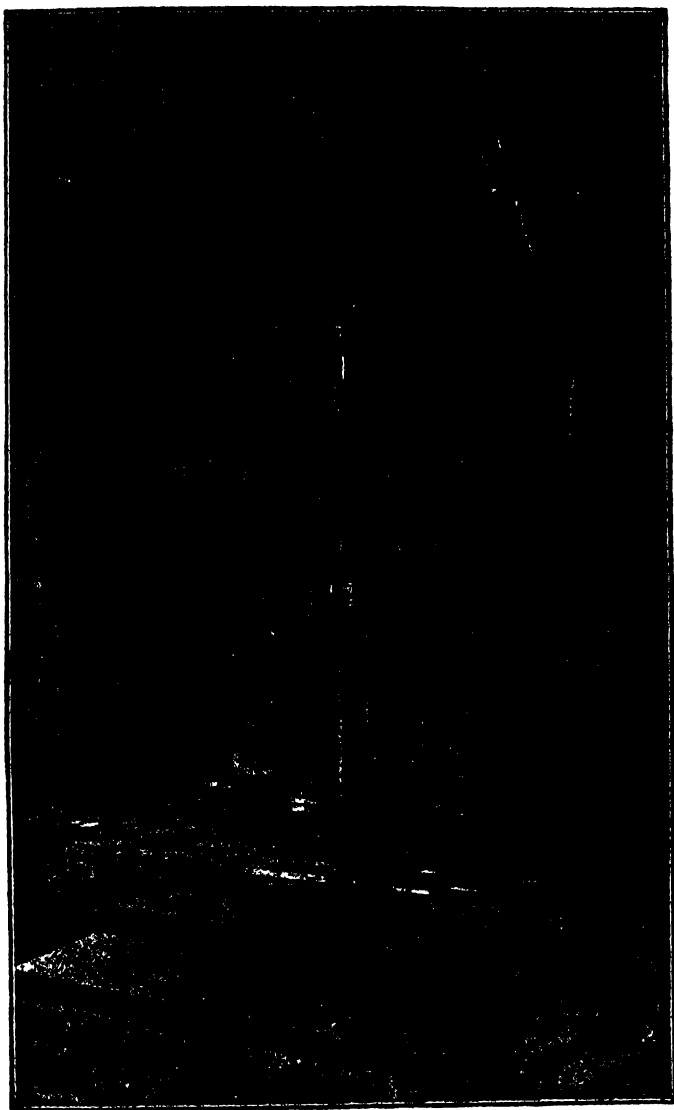
FIG. 6.



series of photographs, which I took by magnesium light, to give a better idea of the appearance of the apparatus and its surroundings. Fig. 5 is a view of the vault showing the clock, the eye end of the big telescope, and the little telescope. In the distant corner is seen the felt screen with a long slit, through which the scale and telescope can be seen from the mirror of the instrument. This, of course, is

on the table behind the screen. Fig. 6 is a view of the corner itself, with the screen drawn back. The octagon protecting house, which

FIG. 7.



surrounds the apparatus, is seen in position. Here again a slit is cut large enough to allow the scale and telescope to be seen from the mirror. Fig. 7 is a view of the instrument with the two halves of

the octagon house separated. Here a further system of screens consisting of concentric brass tubes may be seen, but the lower one, which surrounds the window, has been removed and placed upon the table. The driving gear is also seen in this photograph, and a pipe coming from the screw under the instrument which holds the central tube, which pipe is also seen in Fig. 1. This enables me to control the motion of the mirror from the telescope without approaching the corner in which the apparatus is set up. This is done as follows: the back window at the level of the mirror is made of metal, with a hole in it in which is screwed a metal tube lightly filled with cotton-wool. This is not central, but opposite one end of the mirror. The pipe on the table does not fit the screw, but is merely bent up and enters it loosely. By gently drawing air from the end of the pipe at the telescope a very feeble draught is produced in the apparatus, for nearly all the required air is supplied by leakage round the pipe near the screw, very little entering through the window tube, in consequence of the resistance offered by the cotton-wool. In this way, if the mirror is moving it may be gently brought to rest without impact, or it may be given a swing of any desired amplitude. So perfectly does this work, that the mirror may be steadied very quickly so as to move through less than a scale division, an amount which corresponds to six or seven seconds of arc, or to a force of less than one thousand-millionth of the weight of a grain.

The operations for any complete experiment are fourteen in number. I do not intend to go through these seriatim, as time will not allow me to do so. It is sufficient now to say that the first eight are necessary to get the instrument and scale relatively fixed and adjusted, the vertical measures made, and generally all ready for operation 9, in which the optical compass is employed. This is a most important one, for not only are the horizontal measures made, on which so much depends, but in addition the planes of the wires and fibres are made identical, the corresponding scale reading is found, and any eccentricities are measured and may be corrected.

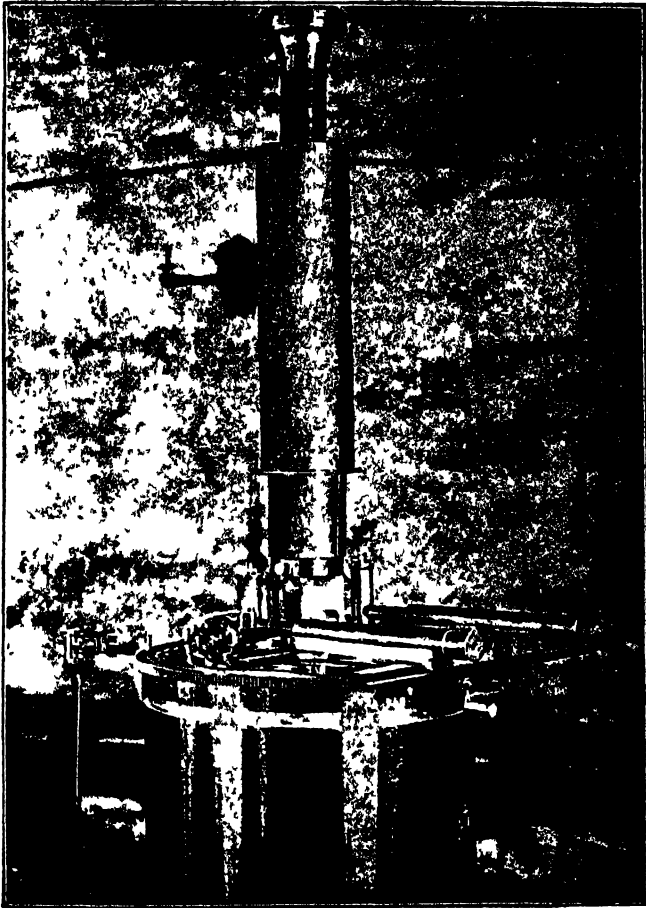
Fig. 8 is a view of the apparatus with the optical compass in position, and with the microscopes focussed upon the wires. They are then ready to be withdrawn by the focussing slide, so as to transfer the distances directly to the small glass scale, as already described.

When this is completed the proper windows are put in position, the screen tubes, the octagon house and the felt screens are all placed ready for operation 10, in which the deflections are measured, and the period with the balls is determined. As this is the operation in which variations of temperature produce so serious an effect, I prefer to leave everything undisturbed for three days, to quiet down. A few hours are quite useless for the purpose.

In operation 11 the period with the counter-weight in the place of the gold balls is measured; also the deflection, if any, due to the lid and lead balls upon the mirror alone. This is only  $1/10$  division,

but its existence is certain. In the later operations the deflections, if any, due to the lid alone on the mirror alone, and to the lid alone on the mirror and gold balls, are separately determined. Neither of these can be detected. The actual elongation of the fibre may also be observed at this stage, but this is of interest only as bearing on the elastic properties of quartz fibres under longitudinal strain.

FIG 8.



Before I come to the treatment of the observations, I should like to refer shortly to the kind of perfection of conditions which by the employment of every practicable refinement that I could devise, I have succeeded in obtaining. Taking experiment 8 as an example, favourable in that the conditions were good, i. e. I was not badly disturbed by trains, wind, or earth tremors, I give the worst and the

best sets of four points of rest obtained from six elongations. They were :—

Worst set + position	Best set — position.
24493	20795·4
24493	20795·7
24493·5	20795·5
24492	20795·5
(24491·7)*	—
24492·9 mean.	20795·5 mean.

Taking all the mean points of rest, as determined above, in groups of three to eliminate slow shifting, if any, of the points of rest, the series of deflections were :—

3696·5  
3696·6  
3696·0  
3696·8

(Interval of one hour, in which oscillations of large amplitude were observed for period.)

(3697·7)  
3696·0

Immediately after the oscillations of large amplitude, which in this case at the end were rather badly disturbed by trains or otherwise, a rather different deflection was observed, but not seriously different. As examination of the figures shows only one anomalous point of rest immediately after the large amplitude disturbance, I feel justified in rejecting the only discordant figure, and in taking the mean of the rest as the true deflection. The unit in this case is 1/10 division. It corresponds to an angular movement of 1/280,000, i.e. about three-quarters of a second of arc. Now a calculation of the angular twist due to a rotation of the air based upon the period, the moment of inertia, and the logarithmic decrement, shows that if the air in the tube were made to whirl round at the rate of one turn in six weeks, so that the air would blow past the gold balls at the rate of one inch in a fortnight, the deflection produced would be 1/10 division, an amount which is greater than the uncertainty of the deflection on this particular night. Again, an examination of the points of rest through the night in the positive and in the negative positions shows a very small steady creep, the same in each case. Creepage of this sort has been, I believe, mentioned as a defect of quartz fibres. When it gives trouble it is due to draughts, as already explained, or to imperfect attachment of the fibres.† In the present instance the creepage observed corresponds to a surface rate of movement on the fibre of a millionth of an inch a month.

\* Disturbed by trains.

† See my paper on 'Attachment of Quartz Fibre,' Phil. Mag. May 1894.



An examination of the mobile system of the beam and suspended gold balls, of which I exhibit a greatly enlarged and working model, at once shows that all the parts are capable of independent movement to an apparently perplexing degree. This in the theory of the instrument I have treated provisionally as a rigid system, moving all as one piece, which it certainly does not seem to be. For instance, the lead balls, by their attraction of the gold balls, pull them out of the perpendicular, so that their distance from the axis is greater than that given by measurement by the optical compass. The error amounts, in the case of the lower ball, when the lead is at its nearest points to  $1/10,000,000$  inch, and I have not taken any notice of it. When the beam is oscillating through so great an angle as  $100,000$  units the centrifugal force only makes the gold ball move out four times as much, and I have taken no notice of that. Again, when the mirror is under acceleration by the fibre, the gold balls, hanging 5 and 11 inches below, do not follow absolutely; they must lag behind, and so affect the period. Now in this case the gold balls are suspended in a manner which is dynamically equivalent to their being at the end of a pendulum  $5\frac{3}{4}$  miles long, the shortest equivalent pendulum that has ever been employed in work of this kind; but short as it is, I have not thought it worth while to be perturbed by an uncertainty of a few inches. There is one point which in some of the experiments only has amounted to a quantity which I do not like to ignore. It is due to the torsional mobility of the separate fibres, about which each gold ball hangs, allowing them in their rotation to slightly lag behind the mirror. As I did not see how to allow for it, I applied to Prof. Greenhill, who immediately told me what to do, and who, with Prof. Minchin, spent a day or two in the country, covering many sheets of paper with logarithms, in finding and solving for me the resulting cubic equation. The correction on this account is  $1/7850$  on the stiffness of the torsion fibre.

There are four remaining corrections depending on the fact that besides the gravitating spheres there are the ball-holders and supporting wires and fibres, all of which produce small but definite disturbances in the gravitation. These are all calculated and allowed for. They are:—

Disturbances due to brass holders of lead balls	..	..	..	..	$1/7320$
" " copper " gold " " " " "	..	..	..	..	$1/265,000$
Attraction of lead balls for quartz fibres	..	..	..	..	$+1/200,000$
" gold " phosphor-bronze wires	..	..	..	..	$-1/115,000$

Then in experiment 9 gold cylinders were employed. Mr. Edser, of the Royal College of Science, calculated for me the correction to be applied if they were treated as spheres; this amounted to  $1/3300$ .

I have already mentioned that experiment 8 was made under more than usually quiet conditions. Such extreme quiet is desirable, that

I manage to reserve Sunday nights, from midnight to six or eight in the morning, for observations of deflection and period. All the other operations can be carried on in the daytime. Sunday is the only night that is suitable, as the railway companies spend every other night shunting and making up trains about a mile away, and this causes such a continuous clatter and vibration, that hours of work may be lost. A passing train does not seem so injurious; but, fortunately for me, most of the observations were made during the coal strike, and fewer trains than usual were running. However, though I may escape from the rattling traffic of St. Giles by working at night, and on Sunday nights am not so badly affected by the trains, I am still not sure of quiet even when there is no wind. For instance, at a quarter to four on Monday morning, Sept. 10, 1893, I was recording chronographically the passage of every ten divisions. Everything was quite quiet, and at the particular moment the marks on the drum recurred at intervals of about three seconds. Suddenly there was a violent non-vibrating lurch of fifteen divisions, or 150 units, which is enormously greater than anything that either trains or traffic could produce; of course I could make no further record. The time of the last mark was, allowing for the known error of the clock, 15h. 44m. 14.3s. This was entered the same day in my note-book as an earthquake, and in Tuesday's *Standard* I read an account of a violent earthquake in Roumania at about the same time. Mr. Charles Davison informs me that the shock was recorded at Bucharest at 15h. 40m. 35s., but that the epicentrum must have been some distance from there. Exact particulars, it seems, cannot be obtained. Though it would appear that the rate of travel of the shock is unusually high, there never was any doubt that what I observed was an earthquake, and it is practically certain that it was the Roumanian earthquake.

Owing to the viscosity of the air, which limits the time during which an observation for period can be made to about 40 minutes, on account of the resistance that the slowly moving mirror and gold balls experience in their passage through it, I made one experiment with a view of reducing this difficulty, by the use of an atmosphere of pure dry hydrogen gas, which possesses a viscosity only half that of air. I found that on this account a great advantage could be gained; but this was more than counterbalanced by the difficulty of getting up a sufficient swing in the gas, and of efficiently controlling the mirror. At the same time, I think that if I had had time to provide means for feeding the gas into the tube without entering the corner, and at the same time were to prevent diffusion at the lower screw, that a little trouble in this direction would be well rewarded. Meantime I found within the limits of error, which were greater than without the hydrogen, that the deflection and the period corrected for the diminished damping were the same. The chief interest of this experiment lies in the fact that it revealed an action unknown to me, and I believe to others, that a thin plane glass mirror, silvered and lacquered on one

side, definitely bends to a small extent, becoming slightly convex on the glass side when in hydrogen, and instantly recovers its form when surrounded by air again. This happened many times, producing a change of focus in the telescope of about  $\frac{1}{8}$  inch. I do not offer any explanation of the fact.

There is an observation which should be of interest to elasticians. In experiments 4 to 8 the torsion fibre carried the beam mirror and the .25-inch gold balls, weighing, with their hooks and fibres, 5.312 grammes. In experiment 9, gold cylinders were substituted, weighing, with their hooks and fibres, 7.976 grammes. The weight of the mirror was .844 gramme. In consequence of the small increase of load the torsional rigidity of the fibre fell more than 4 per cent., an amount far too great to be accounted for by the change of dimensions, even if Poisson's ratio were as great as  $\frac{1}{2}$ . There is no doubt about the great reduction in stiffness, for this figure is one of the factors in the final expression for  $G$ , which does not show a change of more than 1 part in 1570.

It will not be possible at this late hour to explain how the observations are treated so as to obtain the value of  $G$ . It is sufficient to state that in one of these clips all the observed deflections and corrected periods are collected. In the second all the geometrical observations are collected and reduced, so as to obtain what I call the geometrical factor, i.e. a number which, when multiplied by the unknown  $G$ , gives the torsion on the fibre. In the third, the moments of inertia and periods are made use of to find the actual stiffness of the fibre in the several experiments, and in the fourth these are combined so as to find  $G$ . From  $G$  the density of the earth  $\Delta$  immediately follows.

The annexed table contains the important particulars of each experiment. From this it will be seen that the lead balls were twisted and interchanged in every way, so as to show any want of gravitational symmetry if it should exist. For instance, after experiment 7 the ball that was high was made low, the side that was outwards was turned inwards, and their distance apart was reduced by  $\frac{1}{50}$  inch, but the change in the result was less than 1 part in 2400. The experiments 7, 8, 9, 10 were made under widely different circumstances. After experiment 8 the gold balls were changed for heavier gold cylinders, which, as has already been stated, reduced the torsion of the fibre nearly 5 per cent., but the result differs from that of experiment 7 by 1 part in 3700. I then broke the end of the torsion fibre. After keeping it in London three months I broke the other end. I then resoldered each end and put the fibre back in its place, and after making every observation afresh, found with the new shorter and stiffer fibre a result differing from that of experiment 8 by only 1 part in 60,000. These four experiments were all made under favourable circumstances, and on this account I feel more able to rely upon them than on the earlier ones, which were subject to greater uncertainty. The last experiment was made under most unfavourable conditions.

No. of Exp	Lead balls.			Gold balls.		Neutral ind reading.	Date.	Deflection.	Geometrical factor.	Stiffness of fibre.	Result.	
	Arch side.	Wall side.	Shellac spots.	Arch side.	Wall side.						G	A
3	2 low	1 high	Inwards	1·3 grammes each 4 low	3 high	Deg. 267	1892 Oct. 1-30	5637·3	6089·89	·00 245483	·0000000 66645	5·5213
4	2 low	1 high	Inwards	Gold balls of double weight 4 low 3 high		267	1893 Aug. 15- Sept. 3	3667·6	12423·8	772200	66702	5·5167
5	1 high	2 low	Inwards	3 high	4 low	86·5	Sept. 4-11					
6	2 low	1 high	Inwards	4 low	3 high	265·9	Sept. 12-14	3667·7	12422·3	Same as No. 8	66711	5·5159
7	2 low	1 high	Outwards	4 low	3 high	265·9	Sept. 15	3664·0	12432·8		{66675 66551	{5·5189 5·5291
8	1 low	2 high	Inwards	4 low	3 high	265·9	Sept. 16-18	3695·4	12534·2	771664	66579	5·5268
9	1 low	2 high	Inwards	Gold cylinders 3 low 1 high		86	Sept 27- Oct. 3	{5775·5 }	18800·5	739988	66533	5·5306
10	1 low	2 high	Inwards	4 low	3 high	85·25	1894 Jan. 1-13	3515·4	12531·8	811011	66578	5·5269
11	1 low	2 high	Inwards	4 low	3 high	85·25	Jan. 14	3520·5	12533·7	Hydrogen experiment 811385	66695	5·5172
12	2 high	1 low	Inwards	3 high	4 low	265·2	Jan. 17-21					
											Adopted result .. 66576 5·5270	

The periods and deflections were taken in the first four hours after midnight, then, after a few hours' sleep, and far too soon for the temperature to have quieted down, I took the period with the counter-weight, but was only able to give ten minutes, as I had to catch a train in order to be able to give my midday lecture at South Kensington. It is not surprising that under such conditions a difference of 1 part in 600 should arise. There is a difference of about the same order of magnitude between the earlier experiments and the favourable four. There is one point about the figures that I should like to mention. No results were calculated till long after the completion of the last experiment. Had I known how the figures were coming out, it would have been impossible to have been biassed in taking the periods and deflections. Even the calculating boys would not have been quick enough to discover whether the observed elongations were such as would give a definite point of rest. I made my observations, and the figures were copied at once in ink into the books, where afterwards they left my hands and were ground out by the calculating machine. The agreement, such as it is, between my results is therefore in no way the effect of bias, for I had no notion till last May what they would be.

My conclusion is that the force with which two spheres weighing a gramme each, with their centres 1 centimeter apart, attract one another, is  $6.6576 \times 10^{-8}$  dynes, and that the mean density of the earth is 5.5270 times that of water. I do not think the fourth significant figure can be more than one or, at the outside, two parts in error.

It is evident, from what I have already said, that this work is of more than one-man power. Of necessity I am under obligations in many quarters. In the first place, the Department of Science and Art have made it possible for me to carry out the experiment by enabling me to make use of apparatus of my own design. This belongs to the Science Museum, where I hope in time to set it up so that visitors who are interested may observe for themselves the gravitational attraction between small masses. Prof. Clifton, as I have already stated, has given me undisturbed possession of his best observing room, his only good underground room, for the last four years. The late Prof. Pritchard lent me an astronomical clock. Prof. Viriamu Jones enabled me to calibrate the small glass scale on his Whitworth measuring machine; and Mr. Chaney did the same for my weights. I would specially refer to the pains that were taken by Mr. Pye, of the Cambridge Scientific Instrument Company, to carry out every detail as I wished it, and to the highly skilled work of Mr. Colebrook, to which I have already referred. Finally, I am under great obligations to Mr. Starling, of the Royal College of Science, who performed the necessarily tedious calculations.

In conclusion, I have only to say that while I have during the last five years steadily and persistently pursued this one object with the fixed determination to carry it through at any cost, in spite of any

opposition of circumstance, knowing that by my discovery of the value of the quartz fibre, and my development of the design of this apparatus, I had, for the first time, made it possible to obtain the value of Newton's Constant with a degree of accuracy as great as that with which electrical and magnetic units are known; though I have up to the present succeeded to an extent which is greater, I believe, than was expected of me, I am not yet entirely satisfied. I hope to make one more effort this autumn, but the conditions under which I have to work are too difficult; I cannot make the prolonged series of experiments in a spot remote from railways or human disturbance; I cannot escape from that perpetual command to come back to my work in London; so after this I must leave it, feeling sure that the next step can only be made by my methods, but by some one more blest in this world than myself.

[C. V. B.]

Friday, January 18, 1895.

SIR FREDERICK BRAMWELL, BART., D.C.L. LL.D. F.R.S., Honorary  
Secretary and Vice-President, in the Chair.

PROFESSOR DEWAR, M.A. LL.D. F.R.S. M.R.I.

*Phosphorescence and Photographic Action at the Temperature of  
Boiling Liquid Air.*

[Abstract.]

CONTINUED investigation of the properties of matter at extremely low temperatures has resulted in a considerable addition to our knowledge on this subject, more especially in regard to phosphorescence and photographic action. Phosphorescence and fluorescence are terms applied to similar phenomena which apparently differ only in degree, the first being practically an instantaneous effect, while the other lasts for a relatively long period after the withdrawal of the light stimulus. In all cases the luminous effects called phosphorescence and fluorescence belong to a less refrangible part of the spectrum than the exciting rays. Professor Stokes has shown that the singular surface appearance observed in fluorescent liquids is due to a change of refrangibility of the light absorbed, and again given off by their upper layers. Phosphorescence may be regarded as a kind of fluorescence which lasts a long time after the excitation has ceased, and may be briefly defined as the phenomena observed when certain substances give out light through the transformation of absorbed vibrations of shorter period. This must not be confused with the luminosity due to the slow oxidation of phosphorus; nor with the "phosphorescent" appearance accompanying the slow combustion of decaying animal and vegetable matter; nor with the more or less voluntary display of light by fireflies, glow-worms and small marine animals. The researches of Becquerel showed that the intensity of phosphorescence depended directly on the product of the intensity of the stimulating light, and a factor of absorption, and inversely, as some coefficient representing molecular friction or damping. When phosphorescing sulphides of calcium are heated they increase in their light emission, whereas if cooled to  $-80^{\circ}$  they cease altogether to be luminous, and if maintained at this low temperature for hours, keep a latent store of light energy that may be again evolved on allowing the sulphide to rise to the ordinary temperature.

But while the temperature of  $-80^{\circ}$  is sufficient to stop all sensible emission from previously-excited sulphide, it does not prevent an

unexcited sulphide from "absorbing" light energy that can be evolved at higher temperature. By means of liquid air we can now cool substances to temperatures ranging from  $-180^{\circ}$  to  $-200^{\circ}$ . Under such conditions all known organic compounds are solids, and this condition of matter is specially favourable to phosphorescent phenomena. Moreover, the list of truly phosphorescent bodies has been greatly extended, and knowledge of their peculiarities in this direction increased.

The effect of temperature on phosphorescence is easy to observe by taking two portions of the same substance placed in similar very thin test tubes, cooling one of the specimens in liquid air, and then quickly exposing both samples side by side to the same light stimulation. The form of apparatus used is shown in Fig. 1. A is a powerful electric lamp in a lantern, the latter carrying a fitting whereby the light is screened from the eye of the observer. E is

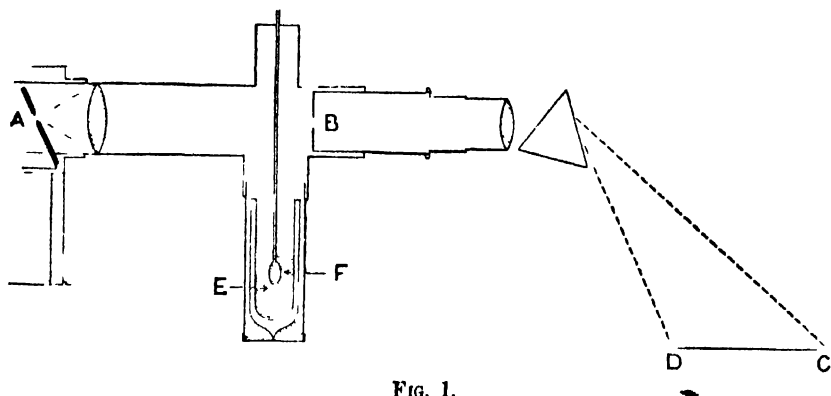


FIG. 1.

a double vacuum vessel containing liquid air or oxygen. The substance to be examined is plunged below the surface of the liquid air, as at F. When it is thoroughly cooled, it is withdrawn from the liquid and exposed for a few seconds to the full light of the arc in the horizontal part of the tube. It is then quickly withdrawn and examined for phosphorescence in the darkened room. The fitting B is another modification of the apparatus for experimental purposes, and consists of a slit and suitable lens and prism, whereby the spectrum can be thrown on to a table, as at D C, and cooled bodies examined in various parts of the spectrum. If, during the light excitation caused by burning magnesium or a flash of the electric light, the eyes are carefully covered, then the comparative phosphorescence, if any, of the cooled and uncooled substance can be observed. In this mode of working the action of the very short wave-lengths of light are stopped by the opacity of glasses, but the solid condition of all substances at the low temperature enables



the use of glass to be abandoned when necessary. As a general rule it may be stated that the great majority of substances exhibiting feeble phosphorescence at ordinary temperature, become markedly more active at these very low temperatures. Thus gelatin, celluloid, paraffine, ivory, horn, and india-rubber become distinctly luminous, with a bluish or greenish phosphorescence, after cooling to  $-180^{\circ}$  and being stimulated by the electric light. Hydroquinone was more luminous than the isomeric resorcinol or pyrocatechol, and in the same way pyrogallol was faint compared with phloroglucol. All alkaloids forming fluorescent solutions become phosphorescent at low temperatures. The hydrocarbons, alcohols, acids and ethers of the fatty series are all more or less active, and glycerin, sulphuric and nitric acids are all very bright, so also are concentrated hydrochloric acid and strong ammonia solution. Coloured salts generally show little activity, but a large number of colourless salts are very luminous. Water when pure is only feebly phosphorescent, but remarkably so when impure. Acetic acid and acetamide appeared fairly equal in luminosity; hippuric acid was very fine, as were most substances containing a ketone group. Lithium platnocyamide changed from white to red on cooling, and was excelled in phosphorescing power by yellow ammonium platinocyanide, which was exceedingly bright.

Definite organic substances possessing exceptional powers of phosphorescence when stimulated at  $-180^{\circ}$  C., are acetophenone, benzophenone, asparagin, hippuric acid, phthalic anhydride, urea, creatine, urethane, succinimide, triphenyl methane, diphenyl, salicylic acid, glycogen, aldehyde-ammonia, &c. It will require long and laborious experiments, however, to measure the relative brightness of the phosphorescence of bodies belonging to definite series.

Remarkable results were obtained with an egg-shell and a feather respectively. The egg shone brilliantly as a globe of blue light, and the feather was equally brilliant, its outline showing clearly in the darkened room. Other organic substances giving good results were cotton-wool, paper, leather, linen, tortoiseshell, and sponge, all phosphorescing brightly, as did also a white flower, a cultivated species of *Dianthus*. Coloured glasses and papers as a rule exhibit no phosphorescence, and when the alcohols are coloured by the addition of a trace of iodine, the luminous effect is destroyed. Milk was shown to be highly phosphorescent and much brighter than water. The white of egg has greater phosphorescing power than the yolk, white substances generally being superior in this respect to coloured ones. On cooling a layer of white of egg on the outside of a test tube to  $-190^{\circ}$ , and then exposing it to a flash of the electric arc, the brilliancy of the phosphorescent light is very striking. The chloro-, bromo-, iodo-, sulfo-, and nitro-compounds, as a rule, show nothing, or are but faintly luminous. Amongst basic bodies nicotine is more luminous than quinoline or pyridine. Metals also phosphoresce, but in this case the action is due to some organic film deposited from the air,

because it disappears on ignition. If the metal is subsequently touched, the phosphorescence re-appears.

So far as the examination has been carried, the two most remarkable classes of substance for phosphorescence are the platinocyanides amongst inorganic compounds, and the ketonic compounds, like acetophenone and ethyl phenyl ketone, and others of the same type, amongst organic. When ammonium platinocyanide is cooled with liquid air and maintained at this temperature by being immersed in the liquid while stimulated by exposure to a beam of the electric arc, it continues to glow in the dark with a feeble emission as long as the temperature is kept about  $-180^{\circ}$ . On pouring off, however, the liquid air from the crystals so that the temperature may rise, then the interior of the test tube glows like a lamp from the sudden increase of light emissivity as the temperature rises. It seems clear

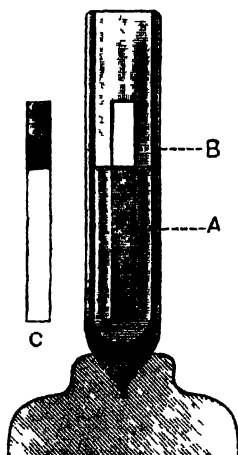


FIG. 2.

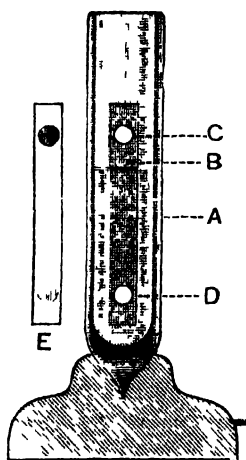


FIG. 3.

from this experiment that similar initial light intensities being used for stimulating, the substance at this low temperature must have acquired increased power of absorption, and it may be that at the same time the factor of molecular friction or damping may have diminished. That the absorptive power of substances for light is greatly changed at low temperatures is proved by the change of colour in substances like oxide, iodide, and sulphide of mercury, chromic acid, &c., when cooled. Many quantitative photometric measurements must be made before the actual changes taking place in the conditions governing the phenomena can be definitely stated.

Along with these experiments on phosphorescence, a number of photographs have been taken at  $-180^{\circ}$ , using various sensitive plates and films, and these have been compared with similar photographs taken at the same time under similar conditions at the ordi-

nary temperature. The first plan (Fig. 2) was simply to immerse a strip of sensitive bromide paper A B, in one of the vacuum vessels containing liquid oxygen, and when the part immersed had been thoroughly cooled down, exposing it to the light of a piece of burning magnesium. The paper was then developed, when a result something resembling C was obtained. The part which had been cooled by the liquid oxygen, as at A, was untouched by the light, whereas the portion of paper above the liquid at B developed up quite black. Further modifications were made in Fig. 3, where the strip of sensitive film E was enclosed in a cover of sheet lead B, having two small discs cut away as at C and D. The strip was then cooled in liquid

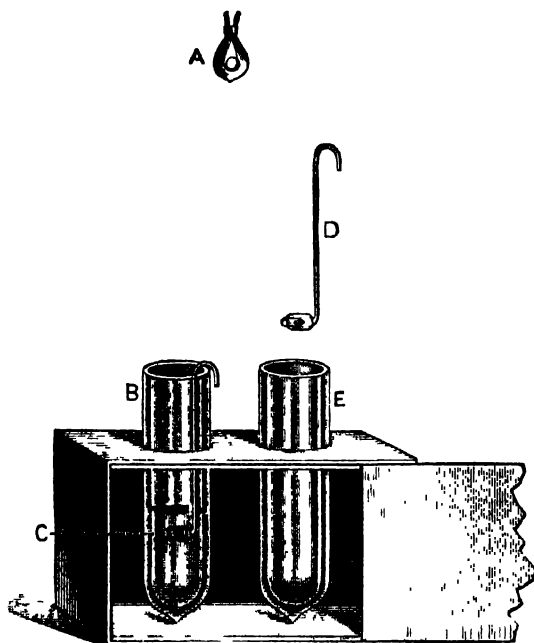


FIG 4.

oxygen A, and then exposed to a flash of burning magnesium. After development, the strip appeared something like E, when again the action of the light was considerably diminished on the part of the film which had been cooled. In Fig. 4 a form of apparatus was adopted whereby the exposures were made without the disadvantage of the light passing through the glass sides of the vacuum vessels. B and E were vacuum vessels enclosed in a blackened box; into B a quantity of liquid oxygen was poured. The sensitive plate or film was then lowered so as just to touch the surface of the liquid as at C. D was a comparison plate exposed at the same time and at the same distance from the source of light A, only the comparison plate

was taken at ordinary temperature. By immersing the photographic plate to different depths in the liquid oxygen or air, the comparative opacities of the liquids could be observed. After exposure, the two plates were removed from the supports and developed together in the same solution, when similar results were obtained as in the previous experiments. The photographs have been examined by Captain Abney, who reports that the photographic action has been reduced by 80 per cent. at the temperature of  $-180^{\circ}$ . If the photographic action is brought about by a chemical change, then it appears to be the only one that can be traced under such conditions, as substances having the most powerful affinities have no action on each other, and all voltaic combinations cease to give a current at such low temperatures. It is certain that the Eastman film, cooled to  $-200^{\circ}$  by the evaporation of air *in vacuo* is still fairly sensitive to photographic action. Much further work, however, will be required to reach a definite conclusion as to what is taking place when substances sensitive to photographic action are subjected to such conditions of temperature.

Friday, March 8, 1895.

SIR FREDERICK ABEL, BART. K.C.B. D.C.L. LL.D. F.R.S.  
Vice-President, in the Chair.

PROFESSOR A. W. RÜCKER, M.A. F.R.S. M.R.I.

*The Physical Work of von Helmholtz.*

THE career we are to consider this evening was a career of singular distinction. In days when the range of "natural knowledge" is so vast that most workers are compelled to be content if they can add something to one or two of the sub-divisions of one of the main branches of science, von Helmholtz showed that it is not impossible to be at once a great mathematician, a great experimental physicist, and, in the widest sense of the term, a great biologist.

It was but eight months yesterday since he delivered his last lecture; it is six months to-day since he died, and the interval is too short for us to attempt to decide on the exact place which will be assigned to him by posterity; but making all allowance for the fact that each age is apt to place its own great among the greatest, making all allowance for the spell which his name cast over many of us in the lecture rooms where we ourselves first gained some knowledge of science, I am sure that I only express the views of all those who know his work best, when I say that we place him in the very front rank of those who have led the great scientific movement of our time. This opinion I have now to justify. I must try to convey to you in some sixty minutes an outline of the work of more than fifty strenuous years, to give you some idea of the wide range of the multifold activities which were crowded into them, of the marvellous insight with which the most diverse problems were attacked and solved, and, if it may be, some image of the man himself. The task is impossible, and I can but attempt some fragments of it.

The history of von Helmholtz is in one respect a simple tale. There are no life and death struggles with fate to record. His work was not done with the wolf at the door, or while he himself was wrestling with disease. He passed through no crises in which success or failure, immortality or oblivion, seemed to depend on the casting of a die. He suffered neither from poverty nor riches. He was a hale, strong man on whom external circumstances neither imposed exceptional disabilities, nor conferred exceptional advantages, but

who, by sheer force of the genius that was in him, passed on from success to success till he was recognised by all as the admirable Crichton of modern science, the most widely cultivated of all students of nature, the acknowledged leader of German science, and one of the first scientific men in the world.

It is the more fitting that this evening should have been set aside for the consideration of the work of Helmholtz, in that England may claim some share in his greatness. Before her marriage his mother bore an English name—Caroline Penn; she was, as her name implied, of English descent. His father was a Professor of Literature in the Gymnasium at Potsdam, so that his early days were passed amid that plain living and high thinking which are characteristic of intellectual circles in Germany. The boy did well at school, and when the time came for choosing a profession, his passion for mathematics and physics had already developed itself. The course of his love for these sciences did not run quite smooth. The path of his ambition was crossed by the hard necessity which in some cases checks, in others fosters, but in all chastens the aspirations of youth. He had to make his livelihood. Science must be to him what the Germans happily call a “bread-study.” Medicine offered a fair prospect of prosperity. Physics, in those days, was but an intellectual pastime. And so the young man took his father’s advice, and became an army doctor. In this, as in so many other cases, “the path of duty was the way to glory.”

It is possible that if von Helmholtz had been what—with a sad consciousness of the limitations it implies—I may call a more physicist, he would have played a greater part in the development of some of those subjects, the study of which he initiated or helped to initiate, but did not thereafter pursue. It is possible that had he been a biologist, and nothing more, he would have followed up the early investigation in which he dealt a blow at the theory that putrefaction and fermentation are chemical processes only, clearly indicating, if he did not actually demonstrate, that the decay which follows death is due to an outburst of low forms of life.

He might thus under other circumstances have done work for which he showed his competence, but which is now chiefly associated with other names; but it is certain that without the unusual combination of wonderful mathematical power and a professional knowledge of anatomy, he would never have accomplished the special tasks which it is his special glory to have achieved.

His first three papers, however, hardly displayed the fusion between his various powers which was afterwards so remarkable a characteristic of his work. The first two were on biological subjects. The third was the famous essay on the ‘Conservation of Force.’ I have told elsewhere the story of the dramatic circumstances under which it was given to the world, of the interest it excited among the members of the Physical Society of Berlin, the refusal of the editor of *Poggendorff’s Annalen* to publish it, and the final triumph of the author

and his views.\* Helmholtz was not, and did not claim to be an original author of the doctrine of the conservation of energy; but two young men, Sir William Thomson in England, and Helmholtz in Germany, independently, and within a month of each other, were the first persons who compelled the scientific world to regard it seriously.

There is one interesting fact which connects this essay directly with the Royal Institution. Four years after it was published, it was placed by Du Bois Reymond in the hands of one who was lost to science in the same year as von Helmholtz himself—the late Prof. Tyndall. He was much impressed, and has spoken of the incident as bringing him face to face with the great doctrine of the “Conservation of Energy.”† He translated the essay into English, and for many years made it his habit to place every physical paper published by Helmholtz within the reach of English readers.

And now, having brought you to the point at which Helmholtz may be said to have been fairly started on his life's work, let me first describe briefly his official career, before I consider his achievements in greater detail.

When his extraordinary abilities became evident, he was permitted to sever his connection with the army. At twenty-seven years of age he became Teacher of Anatomy in the Academy of Arts at Berlin. In the next year he was appointed Professor of Anatomy and Physiology at Königsberg, and he held similar posts in the Universities of Bonn (1855–58) and Heidelberg (1858–71). It was not till 1871 that his early love for physics was finally rewarded. When the chair of Physics was to be filled in the University of the newly-founded German Empire, in Berlin, it was felt that even in Germany—the land of specialists—no better occupant could be found than one who was then in his fiftieth year, and who had been all his life a teacher of anatomy and physiology. The choice was universally approved and completely justified, and von Helmholtz held this post till his death.

In this connection I am, by the kindness of Sir Henry Roscoe, enabled to show to you a relic of remarkable interest. It is a photograph of the great teacher and investigator, taken at the very last lecture that he delivered—that, namely, on July 7, 1894.

For some years, that is, from the date of its foundation, von Helmholtz was the president of the *Physikalisch-Technische Reichsanstalt* in Charlottenburg. This institution, founded partly by the munificence of the late Dr. Werner Siemens, partly by funds supplied by the State, has no precise analogue in this country. It is devoted to the carrying out of systematic researches on questions of fundamental importance to which a long time must be devoted.

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\* *Fortnightly Review*, November, 1894.

† ‘Introduction to Popular Lectures by Helmholtz,’ translated by E. Atkinson, 1873.

The most characteristic work of Helmholtz was, as I have already hinted, that in which his knowledge of physics and his knowledge of anatomy were both directed to a common end. He dealt in turns with the external physical phenomena, with the mechanism of the organs which the phenomena affect, with the relations between the mechanical effect on the organ and the sensations which it excites, and, lastly, with the connection between the sensations in those simple cases which can alone be investigated in the laboratory, and the complex laws of æsthetics and art.

The two books in which these problems were chiefly treated were the 'Physiological Optics,' and the 'Sensations of Sound.' It is impossible to do more than lay before you a sample which may afford some idea of the intricacy of the problems with which he dealt, and of the pitfalls amongst which he walked so warily. For this purpose I have chosen one branch of his work on 'Sound.'

I have deliberately selected that particular portion which has been most questioned, that on which the verdict of most of those who have sat in judgment on his views has been against him.

In discussing this question I must give a general description of the principle phenomena; but if I were to attempt an exhaustive catalogue of all the facts disputed and undisputed, and of all the theories which have been based upon or upset by them, not only would time fail me, but those who have not given special attention to the subject would, I fear, become hopelessly confused amid the chaos of opposing statements and views. Another reason which urges me to be brief, is that a few years ago Prof. Silvanus Thompson explained the whole subject to the members of the Royal Institution, having kindly consented to act as the mouthpiece of the celebrated instrument maker, König, who has played so large a part in these controversies.

Among the chief achievements of Helmholtz was an explanation of the physical difference between pairs of notes which we recognise as concords and discords respectively. When two neighbouring notes are sounded, alternate swellings and fallings off of the intensity are heard which are called beats. These produce an unpleasant effect, which depends partly on their number, partly on the relative pitches of the beating notes. When two notes beat badly, they form an intolerable discord. When they become separated by a wider interval, the beats are so rapid that they cease to be unpleasant.

The sense of dissonance produced by many of these wider intervals, such as the seventh (4 : 7), requires further explanation. In general, the fundamental musical note is only the first and loudest of a series of so-called partials, whose vibration frequencies are 2, 3, 4, &c. times that of the fundamental, and the consonance and dissonance of two notes is shown to depend on the presence or absence of beats between important members of these series. Thus in the case of the seventh the frequencies of the octave of the lower note and that of the upper



note would be in the proportion 8:7, which are sufficiently near to make the beats very prominent and disturbing.

In cases where the notes are pure, that is, are not accompanied by upper partials, the explanation of dissonance is based upon another phenomenon.

When two notes are sounded simultaneously a third tone is often perceived, the frequency of which is equal to the difference of their frequencies. The number of vibrations of this tone is equal to the number of beats, and as there has been controversy as to whether the beats when they become rapid can produce a note, and if so, whether this note is or is not the same thing as the difference tone, it is necessary to distinguish between the two. This distinction is to be found in the mode of their production; but for the moment it is sufficient to remember that they may be distinguishable, and to reserve for them two names, viz. the beat-note, and the first difference tone respectively.

Helmholtz drew attention to the fact that besides the difference tone there is also produced a note, the frequency of which is equal to the sum of those of the two primaries, and this he called the first summation tone.

Together with these he believed that there existed summation and difference tones of higher orders, the whole series being included under the name of combination tones. Our sense of dissonance between pure notes was explained as dependent on beats produced by the combination tones.

Up to the time of Helmholtz it was generally thought that these tones were produced in the ear itself, and had no objective existence in the external air. They are thus often called subjective, but as that adjective is usually reserved for impressions produced in the brain itself, it is better to say that they were regarded as *ear-made*. Helmholtz himself gave a theory, which showed that it is probable that a membrane like the drum-skin of the ear, which is forced out of shape by pressure, and that bones, like those in the ear, which can rattle, would, if acted upon by two notes, manufacture by their own proper movements all the varied combinational tones which his theory postulated. He therefore believed that combinational tones were largely ear-made.

You will observe that his theory of discord is quite unaffected by the question whether the combination tones are or are not sometimes objective. Provided only they are produced at all, it is immaterial whether they are produced in the ear itself. Von Helmholtz admitted that the phenomena we observe are in most cases ear-made tones; but he also asserted that they were sometimes objective, and could set bodies tuned to vibrate with them in resonant motion. This latter statement has been denied with singular unanimity, sometimes, I think, without due regard to the limitations which Helmholtz himself placed on the conditions under which the objective character of the notes can be realised.

All ordinary calculations as to the production and mingling of different waves of sound are based upon the supposition that the displacements of the particles of air, or other body through which the sound is travelling, are very small. If this is so, the force which tends to restore each disturbed particle to its ordinary position of equilibrium is accurately proportional to the amount of the displacement.

In von Helmholtz' view, objective combination tones were in general produced when the disturbance was so great that this condition was no longer fulfilled. Violence is of the essence of the explanation. Hence the siren, where both sets of holes open into the same small wind chest—the harmonium, in which two reeds alternately close and open slits in the same enclosure, are the instruments best suited to produce them. Of these the siren is the more efficient. Von Helmholtz convinced himself that the combination tones produced by the harmonium are for the most part ear-made. He expressly stated that “when the places in which the two tones are struck are entirely separate and have no mechanical connection, as, for example, if they come from two singers, two separate wind instruments, or two violins”—to which we may add two tuning-forks—“the reinforcement of the combinational tones by resonators is small and dubious.” \*

Now this reinforcement by resonators has been altogether denied by most of those who have taken an interest in the matter, while, if an exception is allowed, it is in favour of the beats of a disturbed unison, the observed effects being ascribed to the beats, and not to the difference tone.

Some writers make no exception whatever in their denial of the objective reality of what may be broadly termed secondary tones. Thus Mr. Bosanquet, who made a most careful series of experiments some fourteen years ago, stated that “the ordinary first difference tone . . . is not capable of exciting a resonator. . . . In short, the difference tone of Helmholtz . . . as ordinarily heard, is not objective in its character.” †

Prof. Preyer, too, using very sensitive tuning-forks, found that the differential tone given by two forks did not affect a fork the frequency of which corresponded with its own, except in cases where the difference tone was itself a partial of one of the forks.

It must be remembered that the assertions of Helmholtz as to the experimental proof of the objective nature of the tones were made with reference to those instruments which he regarded as most likely to produce objective notes, viz. the siren and the harmonium, and that, therefore, experiments with forks hardly affect his position.

Let us now try with the siren whether it is possible to confirm or to disprove the validity of his views.

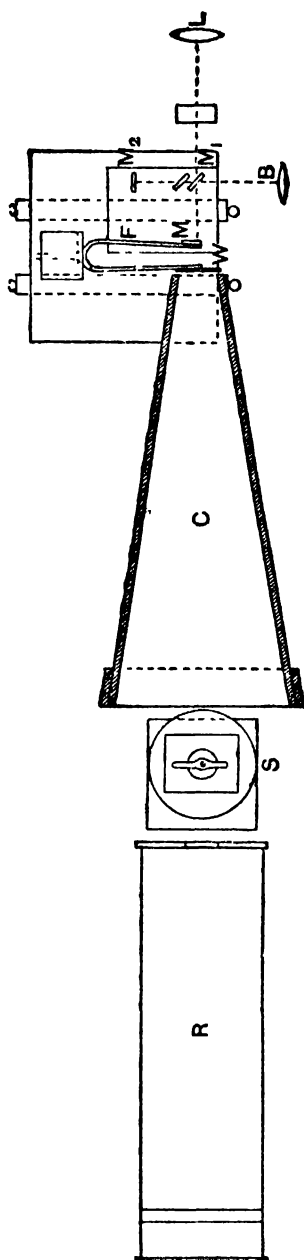
\* ‘Sensations of Tone,’ translated by Ellis, p. 157.

† Proc. Phys. Soc., iv. 1881, p. 233.

For this purpose the rather bulky apparatus which you see before you has been constructed. I should hardly have been able to realise the idea embodied in it, at all events in time to show it to you this evening, if I had not been favourably situated in two respects. In the first place I have had the zealous co-operation of one of my assistants, Mr. Edwin Edser, who has not only made all the parts of the apparatus that required to be newly made, but has thrown himself into the investigation with the utmost energy, working at it late and early, and making many valuable suggestions and improvements. In our joint work we have been helped by some of my senior students, and notably by Messrs. Cullen and Forsyth. In the second place, I have had at my disposal the magnificent collection of acoustical apparatus in the National Museum at South Kensington, some of which I am allowed, by the kindness of the Department of Science and Art, to bring here this evening.

The essential part of the apparatus is a tuning-fork  $F$ , to one prong of which is attached a mirror  $M$ , and to the other a square of thin wood, strengthened by ribs, which is of the same weight as the mirror. The fork thus loaded has been compared with one of König's large standards by means of Lissajous' figures. Its frequency does not differ from 64 complete vibrations per second by more than one vibration in two minutes. The shank is supported by a mass of lead, which in turn is placed upon a paving-stone. Upon this stone also rest the other mirrors necessary for producing Michelson's interference bands. The mirror  $M_1$  is silvered so thinly that half the light which falls upon it is reflected, and half is transmitted.

A ray proceeding from the lantern  $L$  will be divided at  $M_1$  into two, which follow the paths  $LM_1M_2M_1B$  and  $LM_1MM_1B$  respectively. Interference bands are thus produced which can be projected on to a screen, so as to be rendered visible to a large audience.



If the prong of the tuning-fork moves through the eighty-thousandth of an inch, that is, through a distance equal to a half wave-length of light, the path of the ray which falls upon it is shortened by a whole wave-length, and the position of each band is shifted to that previously held by its neighbour. If the fork vibrates with an amplitude of this almost infinitesimal amount, the bands will disappear, or will alternately appear and disappear according to circumstances. The fork may therefore be used to detect by resonance the presence of vibrations, the frequency of which is 64 *per second*.

*A priori*, there were two difficulties of opposite kinds which made it doubtful whether the fork would be an efficient weapon for the purpose for which it was to be used.

In the first place it would feel tremors of any sort, and it was doubtful whether it would be possible to discriminate between mere shakes and the vibrations which were to be studied. This difficulty has been very largely overcome.

The table on which the apparatus stands rests on india-rubber. On the table are a pair of library steps; these support two pieces of wood, which are heavily weighted and rest on india-rubber balls. From these two beams hang steel wires, which carry india-rubber door-fasteners, and these in turn support two rods on which the paving-stone is placed. By this alternation of elastic and of heavy bodies we can make the bands absolutely steady, unless the disturbances are violent. The quiet movements necessary for working the apparatus, the blowing of the bellows, and the like, produce no effect. On the other hand, the shutting of a door in a distant part of the building, the rumble of a cart in the street, will cause the bands to disappear. A great deal of the work on which we rely has been done at South Kensington between midnight and three o'clock in the morning. Trustworthy observations have indeed been made at other times, but it is only in the still small hours that the apparatus is at its best.

The second doubt was of a different kind. It was certain that the instrument would be more or less shaken; it was not quite certain whether the fork would respond to vibrations of the given period. It is easy to set a tuning-fork in vibration by resonance when it is mounted on a sounding box, but in that case the vibrations of the enclosed mass of air are communicated through the box to the fork. When the stalk of the fork is held rigidly, a tuning-fork is notoriously difficult to excite by resonance. This objection is, of course, to some extent counterbalanced by the extraordinary sensitiveness of the means of detecting the vibrations, but it is necessary to supplement this by other devices. The instrument used is a siren (S). In front of it is placed a hollow wooden pyramid, the narrow end of which is near to, and is of the same area as the wooden plate attached to the tuning-fork. This serves to collect the waves of sound and to concentrate them on the fork. Behind the siren is a large resonator by König, timed to respond to 64 vibrations per second.

In some respects the apparatus requires careful handling. Of course if you blow down the connecting cone the fork may be disturbed, and sometimes a particular note of the siren appears to affect the fork for no very obvious reason. Probably the resonance of the air in the cone, or the vibrations of the wooden disc, may at times be the causes of such effects. We have, however, found that whatever they may be due to, they differ in appearance from those produced by vibrations synchronous with the periodic time of the fork, and they can in general be got rid of by a very slight readjustment of the apparatus. The fact that our main conclusions do not depend on any such nicety, is proved by the fact that the instrument has been set up twice in the laboratory, and once in the lecture room in the College. In each case all the experiments have been successful, and on one occasion only were we troubled by a disturbance due to a note (of about 253 vibrations) when sounded alone. A slight readjustment of the cone, however, eliminated this effect entirely.

Such difficulties make it no easy matter to set up the apparatus in a hurry, and the most I can hope to do this evening is to demonstrate to you the methods of using it. I cannot undertake to make the actual measurements before you.

It is, however, desirable to illustrate the sensitiveness of the apparatus to vibrations of 64 per second, and its insensitiveness to other sounds.

Provided the current of air does not travel directly down the cone, organ pipes may be blown just outside it without producing any effect. One of König's large tuning-forks may be bowed strongly without effect.

If, however, the exciting fork be tuned to 64 vibrations per second, and if it be struck as lightly as possible with the handle of a small gimlet, used as a hammer, the handle having been previously covered with india-rubber, the bands will immediately vanish, though the note produced is often quite inaudible, even to a person whose ear is placed close to the fork.

Let the weights on the fork be shifted so that it makes 63.5 vibrations per second, then the resonating fork beats, and the bands regularly appear and disappear every two seconds.

Having thus explained the construction and working of the apparatus, let me show you how we have tested whether it responds to a different tone. When the proper rows of holes are opened, the siren will give simultaneously the  $c'$  of 256 and the  $e'$  of 320 vibrations. The interval is a major third, the difference tone is 64 vibrations. The pitch is determined by the beats between the upper note and a standard tuning-fork which gives  $e'$ . Sounding the upper note alone no effect is produced on the interference bands, as the beats first appear, then die out, and are finally heard again when the note given by the siren is too high.

It could be shown in like manner that the 256-note alone produces no effect, but if, when the standard fork of 320 vibrations and

the upper note of the siren are judged to be in exact accord, the 256-note be also produced, the bands immediately disappear. Sometimes, of course, a small error is made in the estimate of the pitch, and the effect is not instantaneous, but in every case the bands disappear when the beats between the two notes are so slow that they cannot be distinguished.

It is therefore evident that Helmholtz was right when he asserted that the difference tone given by the siren is objective. It exists outside the ear, for it can move a tuning-fork.

König has shown that in many cases, when two notes are sounded simultaneously beats are heard, as though the most prominent phenomenon was the production of beats not between the two fundamental notes, but between the upper of these and the nearest partial of the lower note. Inasmuch as these beats are heard when the lower note (as far as can be tested) is free from upper partials, this rule is not the explanation of the phenomenon, but it is a convenient way of expressing the results. In the experiment just described, the frequencies of the two notes were in the ratio 12 to 15. The first partial of the lower note (12) is therefore the nearest to the higher tone; that is to say, König's beat tone and the first difference tone are identical.

It is easy to arrange an experiment in which these conditions are not fulfilled. Thus let the notes be in the ratio 9:15. The second partial of the lower note is 18, which is nearer to 15 than to 9; hence the König beat-tone would have a relative frequency of  $18 - 15 = 3$ . If the siren rotates  $10.6$  times per second, the frequencies of the two fundamental notes are  $9 \times 10.6 = 96$  and  $15 \times 10.6 = 160$  respectively. As before, the difference tone is 64.

In this case we can use another method of determining the speed of the siren. In 1880 Lord Rayleigh constructed an instrument in which the mass of air enclosed in a tube is excited by resonance, and the fact of the excitation is indicated by a light mirror, which is set where the motion is greatest, inclined at  $45^\circ$  to the direction of the air currents. In accordance with the general law that a lamina tends to place itself perpendicular to the direction of a stream, the mirror moves when the air vibrates. In the original apparatus the amount of the movement was controlled by magnets. Since that date Prof. Boys has modified the instrument by substituting a quartz thread suspension for a silk fibre, and using the torsion of the thread instead of the directing force of the magnets. In a lecture delivered before the British Association, in Leeds, he exhibited the apparatus, which is sometimes called a mirror resonator. Prof. Boys has been good enough to make two of these instruments for me, and for reasons which I will not at the moment enter into, we decided that one of them should respond to 161 vibrations per second. It so happens that this coincides almost exactly with the frequency of one of the notes in the experiment under discussion (160). It is thus possible to use the mirror resonator as an auxiliary instrument to

test the speed of the siren. When the proper note is reached the spot of light will move, and if the difference tone is objective the interference bands ought to disappear simultaneously. We tried this experiment several times. An observer so placed that he could not see the interference bands, lifted his hand when the spot of light moved. It was quite extraordinary to note the absolute agreement between his movements and the behaviour of the bands.

By throwing the spot of light and the bands near together on the screen, the coincidence can be watched by a number of persons. We have tried whether the difference tone is objective in four cases, and in all have detected it by the disappearance of the interference bands. The details of the experiments are collected in the following table. In the first two experiments the first difference tone is, and in the last two it is not, coincident with König's lower beat note.

DIFFERENCE TONES.

Number of holes in siren.	Interval.	Frequencies.		Difference—and König's beat tones.	
15 and 12	Major third	320	256	64	64
16 „ 12	Fourth	256	192	64	64
15 „ 9	Major sixth	160	96	64	32
18 „ 8	{ An octave and a major tone }	115.2	51.2	64	12.8

Of course the question at once arises whether, when it can be distinguished separately, König's beat tone is also objective. I do not wish to express a final opinion on this point, but I may say that when the rows of eight and eighteen holes were opened, the speed of the siren was increased till the notes corresponding to 256 and 576 vibrations were produced. König's note would in that case have a frequency of  $576 - 2 \times 256 = 64$ . We tried twice to obtain this. On the second occasion, especially, all the conditions were favourable, and the experiment was carried on for a long time. On neither occasion did we obtain the smallest sign of an effect on the fork and interference bands.

We must next turn to the summation tone which Helmholtz discovered. It has been almost universally denied that this note is objective. Without going into details, it is only necessary to remark that the late Mr. Ellis, the translator of the 'Tonempfindungen,' who took a dispassionate view of the controversy, thought that the position assumed by Helmholtz had been disproved. To the statement of Helmholtz that "it was formerly believed that the combinational tones were purely subjective and were produced in the ear itself," Ellis appended the note: "the result of Mr. Bosanquet's and Prof. Preyer's quite recent experiments is to show that they are so."

In an experiment on the summation tone, as the total number of vibrations must not exceed 64, the notes will be too low to be well heard. I shall therefore use a third method of determining the rate of speed of the siren. A mirror attached to the lower plate of the instrument rotates with it. Concentric with, and lying on this, is a circle of paper with eighteen cogs. Light reflected from the mirror passes through holes in two pieces of tinfoil attached to the prongs of a tuning-fork. When the fork is at rest, these holes are superposed; but when the fork vibrates, they move apart, are closed by the tinfoil, and only cross each other twice in each complete vibration. The tuning-fork makes 27·2 vibrations per second, and thus allows the light to pass 54·4 times per second. But when the siren makes 3·048 revolutions per second, the rows of nine and twelve holes give a summation tone of 64 vibrations, and each cog moves over  $18 \times 3\cdot048 = 54\cdot9$ , or say 55 times the distance between two consecutive cogs. If the wheel were viewed 55 times a second, the cogs would appear stationary, as in that interval each would be replaced by the next. As they are really seen about 54·4 times a second, they appear to move slowly forwards at the rate of about one interspace in two seconds. When this speed is attained the bands disappear, thus proving the objective existence of the summation tone.

We have repeated this observation in various ways, and always with success. The results are summed up in the table.

It is, perhaps, a drawback that all the notes in these experiments are very low. In order to remedy this, and also to put the matter to the test by means of another instrument, we have employed a mirror resonator which responds to 576 vibrations per second.

The rows of 15 and 12 holes being opened, notes of 320 and 256 vibrations were produced. When they were sounded separately, the noise seemed just to make the resonator move. When they were sounded together, the spot of light was driven off the scale, when the upper note coincided with that of a 320-vibration fork, but immediately returned when this pitch was lost.

SUMMATION TONES.

Numbers of holes in siren.	Interval.	Frequencies.		Sum.
10 and 8	Major third	35·5	28·4	64
12 " 9	Fourth	36·57	27·43	64
16 " 9	Minor seventh	40·96	23·04	64

The summation tone of 576 vibrations was also obtained\* by two other combinations of holes. The 320-fork was used, and the disturbance occurred in the one case when the pitch of the upper note



given by the siren was nearly the same as before, and in the other case when it was about a tone higher.

The results are summed up in the table.

SUMMATION TONES.

Numbers of holes in siren.	Interval.	Frequencies.		Sum.
15 and 12	Major third	320	256	576
16 „ 12	Fourth	329·15	216·85	576
16 „ 9	Major sixth	360	216	576

I venture to think that these experiments prove the accuracy of von Helmholtz. They show that the siren, at all events, does produce objective tones, the frequencies of which coincide with those of the first difference and summation tones, and that this statement is valid as regards the difference tone, whether it is or is not coincident with Konig's beat tone.

I have now in one single case tried to convey to you some idea of the complexity of the problems with which von Helmholtz dealt. He was the first man who detected a relation between the surging mass of partials and combination tones and our sensations of concord and discord. The main facts of his theory are, I believe, generally accepted. On some points modern opinion has tended to stray from his views; one of these we have studied afresh this evening.

It was the fact that I had to deliver this discourse which led me to investigate the question anew, and therefore I felt bound to tell you the results we have at present attained. Had it not been for this, I should not have published them as yet. We have several improvements of the apparatus in view. We do not pretend to have covered the field. I do not, therefore, wish to generalise. My object has been to refute hasty generalisations. I am content if I have convinced you, as I have convinced myself, that Helmholtz was correct in stating that the siren produces objective tones whose frequencies are equal to the sum and difference of their primaries, and that the methods we have employed have brought to light no facts opposed to his view that these notes cannot be explained as secondary effects of partials, but as phenomena of the first order—in other words, as real combination tones.

But brief space now remains to discuss the vast remainder of his work, and as I have already published an appreciation of that,\* I must content myself with trying to give you, in a few sentences, some idea of the range of his intellect.

His investigations on optics were not less important than those

\* 'Fortnightly Review,' November 1894.

on sound. He invented the ophthalmoscope, by which the oculist can study the inmost recesses of the eye. The theory of colour vision, the theory of binocular vision, the curious subjective effects which are produced when we deliberately deceive our own senses by the stereoscope; these subjects he made especially his own.

In the field of mathematics, he was the first to define the peculiar rotatory motion of a liquid known as vortex-motion. Great men had laid the foundations of hydrodynamics before him, but all had overlooked the importance and laws of the vortex. Since the memoir of Helmholtz was published, the subject has been widely studied. Lord Kelvin has originated the famous vortex-ring theory of matter; Prof. Fitzgerald has suggested that the ether may be a complex of vortices, or, as it has been called, a vortex-sponge.

On electricity he wrote much—on the theory of the galvanic cell, on electrolysis, on electromagnetism.

In England, at all events, we give the preference, as regards the last subject, to the theory and writings of our own Maxwell.

As I have already said, von Helmholtz, in an age of specialists, was a universal genius. His intellect could light on nothing which it did not illuminate. Hence, his opinions on side issues are of more than ordinary importance, his "obiter dicta" are worth attention, his popular lectures acquire a special interest. Let us for a few moments turn to these.

The watchword of Helmholtz in dealing with educational problems, is "freedom." Freedom for the student, freedom for the teacher.

In England, we are fond of insisting that there are certain things which everybody who aspires to academic rank must know; of hedging in our students by prescribed courses of study. We make them feel that general culture is an iron-bound safe, which they must wrench open before they can attain the gem of real knowledge, rather than a setting, without which the most profound acquirements seem unattractive and dull. Yet von Helmholtz, one of the most highly educated men, one of the most comprehensive geniuses of the latter end of the century, will have no set courses, except as a preparation for a definite profession, is proud that Germany has "retained the old conception of students, as that of young men responsible to themselves, striving after science of their own free will, to whom it is left to arrange their own plan of studies as they think best." Not content with having made the attainment of this ideal almost impossible for English students, doctrinaire educationalists are now beginning to throw their net around the teacher. It is claimed that as the student must go through a prescribed course of study in order to learn, so the teacher must be drilled and examined before he is allowed to teach. Whatever can be said for this plan as regards the less advanced class of teachers, who are to devote themselves to the instruction of children—and in this case I believe there is something to be said for it—it is quite opposed to von Helmholtz' view of

what is best when the teaching is of university rank, and the students are men and women. Make it easy for whoever has given some proof of knowledge, and wants to teach, to try his hand; make it easy for the student to go to the teacher from whom he gains the most. Look for the best educational results, not necessarily from the best lecturer, but from the man who is in closest contact with his subject. Do not force your teacher on his audience, but do all you can to establish a bond of sympathy between them. Trust, in a word, to the free play of living forces, and not to the hampering restrictions of "necessary subjects" and "compulsory lectures." This is a paraphrase of the views which Helmholtz held, and he illustrated them by the history of this Institution itself.

"I have often," he said, "wondered that the Royal Institution of London, a private society which provides for its members and others short courses of lectures on the progress of natural science, should have been able to retain permanently the services of men of such scientific importance as Humphry Davy and Faraday. It was no question of great emoluments; these men were manifestly attracted by a select public, consisting of men and women of independent mental culture." And then he goes on to show that in a German university the teacher is attracted to his work, because he has to deal with a body of students who are capable of forming opinions, and of judging what is best for themselves.

And this leads us to another point. Von Helmholtz insisted that it is useless and dangerous to crowd the universities with students who are not capable of taking advantage of the opportunities they offer. "The majority of students," he says, "must come to us with a sufficiently logically trained judgment, with a sufficient habit of mental exertion, and with a taste sufficiently developed on the best models to be able to discriminate truth from the bubbling appearance of truth. . . . It would be very dangerous for the universities if large numbers of students frequented them who were less developed in [these] respects. The general self-respect of the students must not be allowed to sink. If that were the case, the dangers of academic freedom would choke its blessings. It must, therefore, not be looked upon as pedantry, or arrogance, if the universities are scrupulous in the admission of students of a different style of education. It would be still more dangerous if, for any extraneous reasons, teachers were introduced into the faculty who have not the complete qualifications of an independent academical teacher." \*

It would be out of place on this occasion to attempt to apply these views to existing circumstances in London; but with the knowledge that the final constitution of a Teaching University for the metropolis may be decided within the next few months, I cannot but feel that London will be happy if it escapes from the fetters which some of its so-called friends are forging for learning; and if, on the other hand,

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\* 'Popular Lectures,' vol. ii. 1881, p. 264-5.

the wise determination of the Gresham Commissioners to include in the university only institutions of university rank, can be maintained against the attacks which will be made upon it.

Lastly, I wish to defend the memory of von Helmholtz from a possible misconception. Those who cultivate art may perhaps look upon him as the poet or the master of style look upon the grammarian; as a mere gerund-grinder, occupied with the study of the dead materials which they alone can use. Of course Helmholtz was not a great artist in the sense that he was a great scientific man, but it would be most unfair to picture him as interested only in the study of law, and as insensitive to beauty; as occupied with sound and light, but careless as to music and painting. I could quote passage after passage from his works to prove his keen sense of the loveliness as well as of the order of nature, to show the homage that he paid, and the freedom he accorded to art. His object was not to lead art captive to science, but rather to unite them in an alliance of mutual confidence and support.

"The horizons of physics, philosophy, and art," he said, "have been too widely separated, and, as a consequence, the language, the methods, and the aims of any one of these studies presents a certain amount of difficulty for the student of any other of them." To smooth away these difficulties, to bridge over the separating gulf, to supply the common language, were the objects of the life work of von Helmholtz. It was a noble ideal, nobly pursued, and crowned with as much success as could reward the efforts of one man. It is an ideal akin to that which dominates this Institution, where science, literature and art are all heard in turns.

If it is possible to sum up in a sentence the teaching of von Helmholtz, and the work of his life, it is that, in spite of the apparent diversities between science and science, between science, philosophy and art there is a fundamental unity, and that the future is for those who detect, amid the seeming discords of the schools, the true harmony which underlies and dominates them all.

[A. W. R.]

Friday, April 5, 1895.

SIR FREDERICK BRAMWELL, Bart. D.C.L. LL.D. F.R.S. Honorary  
Secretary and Vice-President, in the Chair.

The Right Hon. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S.  
Professor of Natural Philosophy, R.I.

*Argon.*

It is some three or four years since I had the honour of lecturing here one Friday evening upon the densities of oxygen and hydrogen gases, and upon the conclusions that might be drawn from the results. It is not necessary, therefore, that I should trouble you to-night with any detail as to the method by which gases can be accurately weighed. I must take that as known, merely mentioning that it is substantially the same as is used by all investigators nowadays, and introduced more than fifty years ago by Regnault. It was not until after that lecture that I turned my attention to nitrogen; and in the first instance I employed a method of preparing the gas which originated with Mr. Vernon Harcourt, of Oxford. In this method the oxygen of ordinary atmospheric air is got rid of with the aid of ammonia. Air is bubbled through liquid ammonia, and then passed through a red-hot tube. In its passage the oxygen of the air combines with the hydrogen of the ammonia, all the oxygen being in that way burnt up and converted into water. The excess of ammonia is subsequently absorbed with acid, and the water by ordinary desiccating agents. That method is very convenient; and, when I had obtained a few concordant results by means of it, I thought that the work was complete, and that the weight of nitrogen was satisfactorily determined. But then I reflected that it is always advisable to employ more than one method, and that the method that I had used—Mr. Vernon Harcourt's method—was not that which had been used by any of those who had preceded me in weighing nitrogen. The usual method consists in absorbing the oxygen of air by means of red-hot copper; and I thought that I ought at least to give that method a trial, fully expecting to obtain forthwith a value in harmony with that already afforded by the ammonia method. The result, however, proved otherwise. The gas obtained by the copper method, as I may call it, proved to be one-thousandth part heavier than that obtained by the ammonia method; and, on repetition, that difference was only brought out more clearly. This was about three years ago. Then, in order, if possible, to get further light upon a discrepancy which

puzzled me very much, and which, at that time, I regarded only with disgust and impatience, I published a letter in 'Nature' inviting criticisms from chemists who might be interested in such questions. I obtained various useful suggestions, but none going to the root of the matter. Several persons who wrote to me privately were inclined to think that the explanation was to be sought in a partial dissociation of the nitrogen derived from ammonia. For, before going further, I ought to explain that, in the nitrogen obtained by the ammonia method, some—about a seventh part—is derived from the ammonia, the larger part, however, being derived as usual from the atmosphere. If the chemically-derived nitrogen were partly dissociated into its component atoms, then the lightness of the gas so prepared would be explained.

The next step in the enquiry was, if possible, to exaggerate the discrepancy. One's instinct at first is to try to get rid of a discrepancy, but I believe that experience shows such an endeavour to be a mistake. What one ought to do is to magnify a small discrepancy with a view to finding out the explanation; and, as it appeared in the present case that the root of the discrepancy lay in the fact that part of the nitrogen prepared by the ammonia method was nitrogen out of ammonia, although the greater part remained of common origin in both cases, the application of the principle suggested a trial of the weight of nitrogen obtained wholly from ammonia. This could easily be done by substituting pure oxygen for atmospheric air in the ammonia method, so that the whole, instead of only a part, of the nitrogen collected should be derived from the ammonia itself. The discrepancy was at once magnified some five times. The nitrogen so obtained from ammonia proved to be about one-half per cent. lighter than nitrogen obtained in the ordinary way from the atmosphere, and which I may call for brevity "atmospheric" nitrogen.

That result stood out pretty sharply from the first; but it was necessary to confirm it by comparison with nitrogen chemically derived in other ways. The Table before you gives a summary of such results, the numbers being the weights in grains actually contained under standard conditions in the globe employed.

#### ATMOSPHERIC NITROGEN.

By hot copper (1892)	.. .. .	2·3103
By hot iron (1893)	.. .. .	2·3100
By ferrous hydrate (1894)	.. .. .	2·3102
	Mean	2·3102

#### CHEMICAL NITROGEN.

From nitric oxide	.. .. .	2·8001
From nitrous oxide	.. .. .	2·2990
From ammonium nitrite purified at a red heat	.. .. .	2·2987
From urea	.. .. .	2·2985
From ammonium nitrite purified in the cold	.. .. .	2·2937
	Mean	2·2990

The difference is about 11 milligrams, or about one-half per cent. ; and it was sufficient to prove conclusively that the two kinds of nitrogen—the chemically-derived nitrogen and the atmospheric nitrogen—differed in weight, and therefore, of course, in quality, for some reason hitherto unknown.

I need not spend time in explaining the various precautions that were necessary in order to establish surely that conclusion. (One had to be on one's guard against impurities, especially against the presence of hydrogen, which might seriously lighten any gas in which it was contained. I believe, however, that the precautions taken were sufficient to exclude all questions of that sort, and the result, which I published about this time last year, stood sharply out, that the nitrogen obtained from chemical sources was different from the nitrogen obtained from the air.

Well, that difference, admitting it to be established, was sufficient to show that some hitherto unknown gas is involved in the matter. It might be that the new gas was dissociated nitrogen, contained in that which was too light, the chemical nitrogen—and at first that was the explanation to which I leaned ; but certain experiments went a long way to discourage such a supposition. In the first place, chemical evidence—and in this matter I am greatly dependent upon the kindness of chemical friends—tends to show that, even if ordinary nitrogen could be dissociated at all into its component atoms, such atoms would not be likely to enjoy any very long continued existence. Even ozone goes slowly back to the more normal state of oxygen ; and it was thought that dissociated nitrogen would have even a greater tendency to revert to the normal condition. The experiment suggested by that remark was as follows : to keep chemical nitrogen—the too light nitrogen which might be supposed to contain dissociated molecules—for a good while, and to examine whether it changed in density. Of course it would be useless to shut up gas in a globe and weigh it, and then, after an interval, to weigh it again, for there would be no opportunity for any change of weight to occur, even although the gas within the globe had undergone some chemical alteration. It is necessary to re-establish the standard conditions of temperature and pressure which are always understood when we speak of filling a globe with gas, for I need hardly say that filling a globe with gas is but a figure of speech. Everything depends upon the temperature and pressure at which you work. However, that obvious point being borne in mind, it was proved by experiment that the gas did not change in weight by standing for eight months—a result tending to show that the abnormal lightness was not the consequence of dissociation.

Further experiments were tried upon the action of the silent electric discharge—both upon the atmospheric nitrogen and upon the chemically-derived nitrogen—but neither of them seemed to be sensibly affected by such treatment ; so that, altogether, the balance of evidence seemed to incline against the hypothesis of abnormal

lightness in the chemically-derived nitrogen being due to dissociation, and to suggest strongly, as almost the only possible alternative, that there must be in atmospheric nitrogen some constituent heavier than true nitrogen.

At that point the question arose, what was the evidence that all the so-called nitrogen of the atmosphere was of one quality? And I remember—I think it was about this time last year, or a little earlier—putting the question to my colleague, Professor Dewar. His answer was that he doubted whether anything material had been done upon the matter since the time of Cavendish, and that I had better refer to Cavendish's original paper. That advice I quickly followed, and I was rather surprised to find that Cavendish had himself put this question quite as sharply as I could put it. Translated from the old-fashioned phraseology connected with the theory of phlogiston, his question was whether the inert ingredient of the air is really all of one kind; whether all the nitrogen of the air is really the same as the nitrogen of nitre. Cavendish not only asked himself this question, but he endeavoured to answer it by an appeal to experiment.

I should like to show you Cavendish's experiment in something like its original form. He inverted a U tube filled with mercury, the legs standing in two separate mercury cups. He then passed up, so as to stand above the mercury, a mixture of nitrogen, or of air, and oxygen; and he caused an electric current from a frictional electrical machine like the one I have before me to pass from the mercury in the one leg to the mercury in the other, giving sparks across the intervening column of air. I do not propose to use a frictional machine to-night, but I will substitute for it one giving electricity of the same quality of the construction introduced by Mr. Wimshurst, of which we have a fine specimen in the Institution. It stands just outside the door of the theatre, and will supply an electric current along insulated wires, leading to the mercury cups; and, if we are successful, we shall cause sparks to pass through the small length of air included above the columns of mercury. There they are; and after a little time you will notice that the mercury rises, indicating that the gas is sensibly absorbed under the influence of the sparks and of a piece of potash floating on the mercury. It was by that means that Cavendish established his great discovery of the nature of the inert ingredient in the atmosphere, which we now call nitrogen; and, as I have said, Cavendish himself proposed the question, as distinctly as we can do, is this inert ingredient all of one kind? and he proceeded to test that question. He found, after days and weeks of protracted experiment, that, for the most part, the nitrogen of the atmosphere was absorbed in this manner, and converted into nitrous acid; but that there was a small residue remaining after prolonged treatment with sparks, and a final absorption of the residual oxygen. That residue amounted to about  $\frac{1}{20}$  part of the nitrogen taken; and Cavendish draws the conclusion



that, if there be more than one inert ingredient in the atmosphere, at any rate the second ingredient is not contained to a greater extent than  $\frac{1}{128}$  part.

I must not wait too long over the experiment. Mr Gordon tells me that a certain amount of contraction has already occurred; and if we project the U upon the screen, we shall be able to verify the fact. It is only a question of time for the greater part of the gas to be taken up, as we have proved by preliminary experiments.

In what I have to say from this point onwards, I must be understood as speaking as much on behalf of Professor Ramsay as for myself. At the first, the work which we did was to a certain extent independent. Afterwards we worked in concert, and all that we have published in our joint names must be regarded as being equally the work of both of us. But, of course, Professor Ramsay must not be held responsible for any chemical blunder into which I may stumble to-night.

By his work and by mine the heavier ingredient in atmospheric nitrogen which was the origin of the discrepancy in the densities has been isolated, and we have given it the name of "argon." For this purpose we may use the original method of Cavendish, with the advantages of modern appliances. We can procure more powerful electric sparks than any which Cavendish could command by the use of the ordinary Ruhmkorff coil stimulated by a battery of Grove cells; and it is possible so to obtain evidence of the existence of argon. The oxidation of nitrogen by that method goes on pretty quickly. If you put some ordinary air, or, better still, a mixture of air and oxygen, in a tube in which electric sparks are made to pass for a certain time, then in looking through the tube, you observe the well-known reddish-orange fumes of the oxides of nitrogen. I will not take up time in going through the experiment, but will merely exhibit a tube already prepared (image on screen).

One can work more efficiently by employing the alternate currents from dynamo machines which are now at our command. In this Institution we have the advantage of a public supply; and if I pass alternate currents originating in Deptford through this Ruhmkorff coil, which acts as what is now called a "high potential transformer," and allow sparks from the secondary to pass in an inverted test tube between platinum points, we shall be able to show in a comparatively short time a pretty rapid absorption of the gases. The electric current is led into the working chamber through bent glass tubes containing mercury, and provided at their inner extremities with platinum points. In this arrangement we avoid the risk, which would otherwise be serious, of a fracture just when we least desired it. I now start the sparks by switching on the Ruhmkorff to the alternate current supply; and, if you will take note of the level of the liquid representing the quantity of mixed gases included, I think you will see after, perhaps, a quarter of an hour that the liquid has

very appreciably risen, owing to the union of the nitrogen and the oxygen gases under the influence of the electrical discharge, and subsequent absorption of the resulting compound by the alkaline liquid with which the gas space is enclosed.

By means of this little apparatus, which is very convenient for operations upon a moderate scale, such as for analyses of "nitrogen" for the amount of argon that it may contain, we are able to get an absorption of about 80 cubic centimetres per hour, or about 4 inches along this test tube, when all is going well. In order, however, to obtain the isolation of argon on any considerable scale by means of the oxygen method, we must employ an apparatus still more enlarged. The isolation of argon requires the removal of nitrogen, and, indeed, of very large quantities of nitrogen, for, as it appears, the proportion of argon contained in atmospheric nitrogen is only about 1 per cent., so that for every litre of argon that you wish to get you must eat up some hundred litres of nitrogen. That, however, can be done upon an adequate scale by calling to our aid the powerful electric discharge now obtainable by means of the alternate current supply and high potential transformers.

In what I have done upon this subject I have had the advantage of the advice of Mr. Crookes, who some years ago drew special attention to the electric discharge or flame, and showed that many of its properties depended upon the fact that it had the power of causing, upon a very considerable scale, a combination of the nitrogen and the oxygen of the air in which it was made.

I had first thought of showing in the lecture room the actual apparatus which I have employed for the concentration of argon; but the difficulty is that, as the apparatus has to be used, the working parts are almost invisible, and I came to the conclusion that it would really be more instructive as well as more convenient to show the parts isolated, a very little effort of imagination being then all that is required in order to reconstruct in the mind the actual arrangements employed.

First, as to the electric arc or flame itself. We have here a transformer made by Pike and Harris. It is not the one that I have used in practice; but it is convenient for certain purposes, and it can be connected by means of a switch with the alternate currents of 100 volts furnished by the Supply Company. The platinum terminals that you see here are modelled exactly upon the plan of those which have been employed in practice. I may say a word or two on the question of mounting. The terminals require to be very massive on account of the heat evolved. In this case they consist of platinum wire doubled upon itself six times. The platinums are continued by iron wires going through glass tubes, and attached at the ends to the copper leads. For better security, the tubes themselves are stopped at the lower ends with corks and charged with water, the advantage being that, when the whole arrangement is fitted by means of an indiarubber stopper into a closed vessel, you have a witness that, as

long as the water remains in position, no leak can have occurred through the insulating tubes conveying the electrodes.

Now, if we switch on the current and approximate the points sufficiently, we get the electric flame. There you have it. It is, at present, showing a certain amount of soda. That in time would burn off. After the arc has once been struck, the platimums can be separated; and then you have two tongues of fire ascending almost independently of one another, but meeting above. Under the influence of such a flame, the oxygen and the nitrogen of the air combine at a reasonable rate, and in this way the nitrogen is got rid of. It is now only a question of boxing up the gas in a closed space, where the argon concentrated by the combustion of the nitrogen can be collected. But there are difficulties to be encountered here. One cannot well use anything but a glass vessel. There is hardly any metal available that will withstand the action of strong caustic alkali and of the nitrous fumes resulting from the flame. One is practically limited to glass. The glass vessel employed is a large flask with a single neck, about half full of caustic alkali. The electrodes are carried through the neck by means of an indiarubber bung provided also with tubes for leading in the gas. The electric flame is situated at a distance of only about half an inch above the caustic alkali. In that way an efficient circulation is established; the hot gases as they rise from the flame strike the top, and then as they come round again in the course of the circulation they pass sufficiently close to the caustic alkali to ensure an adequate removal of the nitrous fumes.

There is another point to be mentioned. It is necessary to keep the vessel cool; otherwise the heat would soon rise to such a point that there would be excessive generation of steam, and then the operation would come to a standstill. In order to meet this difficulty the upper part of the vessel is provided with a water-jacket, in which a circulation can be established. No doubt the glass is severely treated, but it seems to stand it in a fairly amiable manner.

By means of an arrangement of this kind, taking nearly three horse-power from the electric supply, it is possible to consume nitrogen at a reasonable rate. The transformers actually used are the "Hedgehog" transformers of Mr. Swinburne, intended to transform from 100 volts to 2400 volts. By Mr. Swinburne's advice I have used two such, the fine wires being in series so as to accumulate the electrical potential and the thick wires in parallel. The rate at which the mixed gases are absorbed is about seven litres per hour; and the apparatus, when once fairly started, works very well as a rule, going for many hours without attention. At times the arc has a trick of going out, and it then requires to be restarted by approximating the platimums. We have already worked 14 hours on end, and by the aid of one or two automatic appliances it would, I think, be possible to continue operations day and night.

The gases, air and oxygen in about equal proportions, are mixed

in a large gasholder, and are fed in automatically as required. The argon gradually accumulates; and when it is desired to stop operations the supply of nitrogen is cut off, and only pure oxygen allowed admittance. In this way the remaining nitrogen is consumed, so that, finally, the working vessel is charged with a mixture of argon and oxygen only, from which the oxygen is removed by ordinary well-known chemical methods. I may mention that at the close of the operation, when the nitrogen is all gone, the arc changes its appearance, and becomes of a brilliant blue colour.

I have said enough about this method, and I must now pass on to the alternative method which has been very successful in Professor Ramsay's hands—that of absorbing nitrogen by means of red-hot magnesium. By the kindness of Professor Ramsay and Mr. Matthews, his assistant, we have here the full scale apparatus before us almost exactly as they use it. On the left there is a reservoir of nitrogen derived from air by the simple removal of oxygen. The gas is then dried. Here it is bubbled through sulphuric acid. It then passes through a long tube made of hard glass and charged with magnesium in the form of thin turnings. During the passage of the gas over the magnesium at a bright red heat, the nitrogen is absorbed in a great degree, and the gas which finally passes through is immensely richer in argon than that which first enters the hot tube. At the present time you see a tolerably rapid bubbling on the left, indicative of the flow of atmospheric nitrogen into the combustion furnace; whereas, on the right, the outflow is very much slower. Care must be taken to prevent the heat rising to such a point as to soften the glass. The concentrated argon is collected in a second gasholder, and afterwards submitted to further treatment. The apparatus employed by Professor Ramsay in the subsequent treatment is exhibited in the diagram, and is very effective for its purpose; but I am afraid that the details of it would not readily be followed from any explanation that I could give in the time at my disposal. The principle consists in the circulation of the mixture of nitrogen and argon over hot magnesium, the gas being made to pass round and round until the nitrogen is effectively removed from it. At the end that operation, as in the case of the oxygen method, proceeds somewhat slowly. When the greater part of the nitrogen is gone, the remainder seems to be unwilling to follow, and it requires somewhat protracted treatment in order to be sure that the nitrogen has wholly disappeared. When I say "wholly disappeared," that, perhaps, would be too much to say in any case. What we can say is that the spectrum test is adequate to show the presence, or at any rate to show the addition, of about one-and-a-half per cent. of nitrogen to argon as pure as we can get it; so that it is fair to argue that any nitrogen at that stage remaining in the argon is only a small fraction of one-and-a-half per cent.

I should have liked at this point to be able to give advice as to which of the two methods—the oxygen method or the magnesium

method—is the easier and the more to be recommended ; but I confess that I am quite at a loss to do so. One difficulty in the comparison arises from the fact that they have been in different hands. As far as I can estimate, the quantities of nitrogen eaten up in a given time are not very different. In that respect, perhaps, the magnesium method has some advantage ; but, on the other hand, it may be said that the magnesium process requires a much closer supervision, so that, perhaps, fourteen hours of the oxygen method may not unfairly compare with eight hours or so of the magnesium method. In practice a great deal would depend upon whether in any particular laboratory alternate currents are available from a public supply. If the alternate currents are at hand, I think it may probably be the case that the oxygen method is the easier ; but, otherwise, the magnesium method would, probably, be preferred, especially by chemists who are familiar with operations conducted in red-hot tubes.

I have here another experiment illustrative of the reaction between magnesium and nitrogen. Two rods of that metal are suitably mounted in an atmosphere of nitrogen, so arranged that we can bring them into contact and cause an electric arc to form between them. Under the action of the heat of the electric arc the nitrogen will combine with the magnesium ; and if we had time to carry out the experiment we could demonstrate a rapid absorption of nitrogen by this method. When the experiment was first tried, I had hoped that it might be possible, by the aid of electricity, to start the action so effectively that the magnesium would continue to burn independently under its own developed heat in the atmosphere of nitrogen. Possibly, on a larger scale, something of this sort might succeed, but I bring it forward here only as an illustration. We turn on the electric current, and bring the magnesiums together. You see a brilliant green light, indicating the vaporisation of the magnesium. Under the influence of the heat the magnesium burns, and there is collected in the glass vessel a certain amount of brownish-looking powder which consists mainly of the nitride of magnesium. Of course, if there is any oxygen present it has the preference, and the ordinary white oxide of magnesium is formed.

The gas thus isolated is proved to be inert by the very fact of its isolation. It refuses to combine under circumstances in which nitrogen, itself always considered very inert, does combine—both in the case of the oxygen treatment and in the case of the magnesium treatment ; and these facts are, perhaps, almost enough to justify the name which we have suggested for it. But, in addition to this, it has been proved to be inert under a considerable variety of other conditions such as might have been expected to tempt it into combination. I will not recapitulate all the experiments which have been tried, almost entirely by Professor Ramsay, to induce the gas to combine. Hitherto, in our hands, it has not done so ; and I may mention that recently, since the publication of the abstract of our paper read

before the Royal Society, argon has been submitted to the action of titanium at a red heat, titanium being a metal having a great affinity for nitrogen, and that argon has resisted the temptation to which nitrogen succumbs. We never have asserted, and we do not now assert, that argon can under no circumstances be got to combine. That would, indeed, be a rash assertion for any one to venture upon; and only within the last few weeks there has been a most interesting announcement by M. Berthelot, of Paris, that, under the action of the silent electric discharge, argon can be absorbed when treated in contact with the vapour of benzine. Such a statement, coming from so great an authority, commands our attention; and if we accept the conclusion, as I suppose we must do, it will follow that argon has, under those circumstances, combined.

Argon is rather freely soluble in water. That is a thing that troubled us at first in trying to isolate the gas; because, when one was dealing with very small quantities, it seemed to be always disappearing. In trying to accumulate it we made no progress. After a sufficient quantity had been prepared, special experiments were made on the solubility of argon in water. It has been found that argon, prepared both by the magnesium method and by the oxygen method, has about the same solubility in water as oxygen—some two-and-a-half times the solubility of nitrogen. This suggests, what has been verified by experiment, that the dissolved gases of water should contain a larger proportion of argon than does atmospheric nitrogen. I have here an apparatus of a somewhat rough description, which I have employed in experiments of this kind. The boiler employed consists of an old oil-can. The water is supplied to it and drawn from it by coaxial tubes of metal. The incoming cold water flows through the outer annulus between the two tubes. The outgoing hot water passes through the inner tube, which ends in the interior of the vessel at a higher level. By means of this arrangement the heat of the water which has done its work is passed on to the incoming water not yet in operation, and in that way a limited amount of heat is made to bring up to the boil a very much larger quantity of water than would otherwise be possible, the greater part of the dissolved gases being liberated at the same time. These are collected in the ordinary way. What you see in this flask is dissolved air collected out of water in the course of the last three or four hours. Such gas, when treated as if it were atmospheric nitrogen, that is to say after removal of the oxygen and minor impurities, is found to be decidedly heavier than atmospheric nitrogen to such an extent as to indicate that the proportion of argon contained is about double. It is obvious, therefore, that the dissolved gases of water form a convenient source of argon, by which some of the labour of separation from air is obviated. During the last few weeks I have been supplied from Manchester by Mr. Macdougall, who has interested himself in this matter, with a quantity of dissolved gases obtained from the condensing water of his steam engine.

As to the spectrum, we have been indebted from the first to Mr. Crookes, and he has been good enough to-night to bring some tubes which he will operate, and which will show you at all events the light of the electric discharge in argon. I cannot show you the spectrum of argon, for unfortunately the amount of light from a vacuum tube is not sufficient for the projection of its spectrum. Under some circumstances the light is red, and under other circumstances it is blue. Of course when these lights are examined with the spectroscope--and they have been examined by Mr. Crookes with great care--the differences in the colour of the light translate themselves into different groups of spectrum lines. We have before us Mr. Crookes' map, showing the two spectra upon a very large scale. The upper is the spectrum of the blue light; the lower is the spectrum of the red light; and it will be seen that they differ very greatly. Some lines are common to both; but a great many lines are seen only in the red, and others are seen only in the blue. It is astonishing to notice what trifling changes in the conditions of the discharge bring about such extensive alterations in the spectrum.

One question of great importance, upon which the spectrum throws light is, is the argon derived by the oxygen method really the same as the argon derived by the magnesium method? By Mr. Crookes' kindness I have had an opportunity of examining the spectra of the two gases side by side, and such examination as I could make revealed no difference whatever in the two spectra, from which, I suppose, we may conclude either that the gases are absolutely the same, or, if they are not the same, that at any rate the ingredients by which they differ cannot be present in more than a small proportion in either of them.

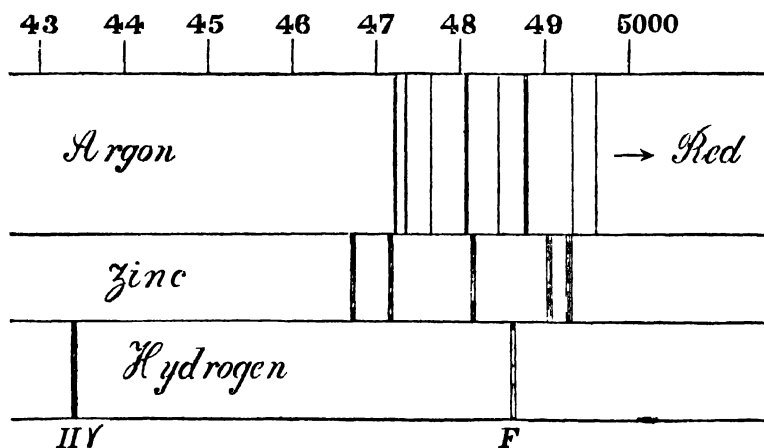
My own observations upon the spectrum have been made principally at atmospheric pressure. In the ordinary process of sparking, the pressure is atmospheric; and, if we wish to look at the spectrum, we have nothing more to do than to include a jar in the circuit, and to put a direct-vision prism to the eye. At my request, Professor Schuster examined some tubes containing argon at atmospheric pressure prepared by the oxygen method, and I have here a diagram of a characteristic group. He also placed upon the sketch some of the lines of zinc, which were very convenient as directing one exactly where to look. See figure on page 408.

Within the last few days, Mr. Crookes has charged a radiometer with argon. When held in the light from the electric lamp, the vanes revolve rapidly. Argon is anomalous in many respects, but not, you see, in this.

Next, as to the density of argon. Professor Ramsay has made numerous and careful observations upon the density of the gas prepared by the magnesium method, and he finds a density of about 19.9 as compared with hydrogen. Equally satisfactory observations upon the gas derived by the oxygen method have not yet been made,

but there is no reason to suppose that the density is different, such numbers as 19.7 having been obtained.

One of the most interesting matters in connection with argon, however, is what is known as the ratio of the specific heats. I must not stay to elaborate the questions involved, but it will be known to many who hear me that the velocity of sound in a gas depends upon the ratio of two specific heats—the specific heat of the gas measured at constant pressure, and the specific heat measured at constant volume. If we know the density of a gas, and also the velocity of sound in it, we are in a position to infer this ratio of specific heats; and, by means of this method, Professor Ramsay has determined the ratio in the case of argon, arriving at the very remarkable result that the ratio of specific heats is represented by the number 1.65, approaching very closely to the theoretical limit, 1.67. The number



1.67 would indicate that the gas has no energy except energy of translation of its molecules. If there is any other energy than that, it would show itself by this number dropping below 1.67. Ordinary gases, oxygen, nitrogen, hydrogen, &c., do drop below, giving the number 1.4. Other gases drop lower still. If the ratio of specific heats is 1.65, practically 1.67, we may infer then that the whole energy of motion is translational; and from that it would seem to follow by arguments which, however, I must not stop to elaborate, that the gas must be of the kind called by chemists monatomic.

I had intended to say something of the operation of determining the ratio of specific heats, but time will not allow. The result is, no doubt, very awkward. Indeed, I have seen some indications that the anomalous properties of argon are brought as a kind of accusation against us. But we had the very best intentions in the matter. The facts were too much for us; and all we can do now is to apologise for ourselves and for the gas.



Several questions may be asked, upon which I should like to say a word or two, if you will allow me to detain you a little longer. The first question (I do not know whether I need ask it) is, have we got hold of a new gas at all? I had thought that that might be passed over, but only this morning I read in a technical journal the suggestion that argon was our old friend nitrous oxide. Nitrous oxide has, roughly, the density of argon; but that, as far as I can see, is the only point of resemblance between them.

Well, supposing that there is a new gas, which I will not stop to discuss, because I think the spectrum alone would be enough to prove it, the next question that may be asked is, is it in the atmosphere? This matter naturally engaged our earnest attention at an early stage of the enquiry. I will only indicate in a few words the arguments which seem to us to show that the answer must be in the affirmative.

In the first place, if argon be not in the atmosphere, the original discrepancy of densities which formed the starting point of the investigation remains unexplained, and the discovery of the new gas has been made upon a false clue. Passing over that, we have the evidence from the blank experiments, in which nitrogen originally derived from chemical sources is treated either with oxygen or with magnesium, exactly as atmospheric nitrogen is treated. If we use atmospheric nitrogen, we get a certain proportion of argon, about 1 per cent. If we treat chemical nitrogen in the same way we get, I will not say absolutely nothing, but a mere fraction of what we should get had atmospheric nitrogen been the subject. You may ask, why do we get any fraction at all from chemical nitrogen? It is not difficult to explain the small residue, because in the manipulation of the gases large quantities of water are used; and, as I have already explained, water dissolves argon somewhat freely. In the processes of manipulation some of the argon will come out of solution, and it remains after all the nitrogen has been consumed.

Another wholly distinct argument is founded upon the method of diffusion introduced by Graham. Graham showed that if you pass gas along porous tubes you alter the composition, if the gas is a mixture. The lighter constituents go more readily through the pores than do the heavier ones. The experiment takes this form. A number of tobacco pipes—eight in the actual arrangement—are joined together in series with indiarubber junctions, and they are put in a space in which a vacuum can be made, so that the space outside the porous pipes is vacuous or approximately so. Through the pipes ordinary air is led. One end may be regarded as open to the atmosphere. The other end is connected with an aspirator so arranged that the gas collected is only some 2 per cent. of that which leaks through the porosities. The case is like that of an Australian river drying up almost to nothing in the course of its flow. Well, if we treat air in that way, collecting only the small residue which is less willing than the remainder to penetrate the porous walls, and then

prepare "nitrogen" from it by removal of oxygen and moisture, we obtain a gas heavier than atmospheric nitrogen, a result which proves that the ordinary nitrogen of the atmosphere is not a simple body, but is capable of being divided into parts by so simple an agent as the tobacco pipe.

If it be admitted that the gas is in the atmosphere, the further question arises as to its nature.

At this point I would wish to say a word of explanation. Neither in our original announcement at Oxford, nor at any time since, until the 31st of January, did we utter a word suggesting that argon was an element; and it was only after the experiments upon the specific heats that we thought we had sufficient to go upon in order to make any such suggestion in public. I will not insist that that observation is absolutely conclusive. It is certainly strong evidence. But the subject is difficult, and one that has given rise to some difference of opinion among physicists. At any rate this property distinguishes argon very sharply from all the ordinary gases.

One question which occurred to us at the earliest stage of the enquiry, as soon as we knew that the density was not very different from 21, was the question of whether, possibly, argon could be a more condensed form of nitrogen, denoted chemically by the symbol  $N_3$ . There seem to be several difficulties in the way of this supposition. Would such a constitution be consistent with the ratio of specific heats (1.65)? That seems extremely doubtful. Another question is, can the density be really as high as 21, the number required on the supposition of  $N_3$ ? As to this matter, Professor Ramsay has repeated his measurements of density, and he finds that he cannot get even so high as 20. To suppose that the density of argon is really 21, and that it appears to be 20 in consequence of nitrogen still mixed with it, would be to suppose a contamination with nitrogen out of all proportion to what is probable. It would mean some 14 per cent. of nitrogen, whereas it seems that from one-and-a-half to two per cent. is easily enough detected by the spectroscope. Another question that may be asked is, would  $N_3$  require so much cooling to condense it as argon requires?

There is one other matter on which I would like to say a word—the question as to what  $N_3$  would be like if we had it. There seems to be a great discrepancy of opinions. Some high authorities, among whom must be included, I see, the celebrated Mendeleef, consider that  $N_3$  would be an exceptionally stable body; but most of the chemists with whom I have consulted are of opinion that  $N_3$  would be explosive, or, at any rate, absolutely unstable. That is a question which may be left for the future to decide. We must not attempt to put these matters too positively. The balance of evidence still seems to be against the supposition that argon is  $N_3$ , but for my part I do not wish to dogmatise.

A few weeks ago we had an eloquent lecture from Professor

Rücker on the life and work of the illustrious Helmholtz. It will be known to many that during the last few months of his life Helmholtz lay prostrate in a semi-paralysed condition, forgetful of many things, but still retaining a keen interest in science. Some little while after his death we had a letter from his widow, in which she described how interested he had been in our preliminary announcement at Oxford upon this subject, and how he desired the account of it to be read to him over again. He added the remark, "I always thought that there must be something more in the atmosphere."

Friday, April 26, 1895.

SIR FREDERICK BRAMWELL, BART. D.C.L. LL.D. F.R.S. Honorary  
Secretary and Vice-President, in the Chair.

JOHN HOPKINSON, Esq. M.A. D.Sc. F.R.S. M.R.I.

*The Effects of Electric Currents in Iron on its Magnetisation.*

LET us recall a well-known experiment of Faraday's. Upon a ring of iron is wound a few turns of copper wire, through which may be passed a current. On it is so wound a second coil, the ends of which are connected to a galvanometer. It was known that when a current is passed through the first-named or primary coil, the ring becomes a magnet, and that if the current is reversed in direction the magnetisation is also reversed. Faraday showed that when the magnetism is reversed, a transient current is caused in the second coil. The current which is reversed is a measure of the magnetising force; the total amount of current caused in the second coil by the reversal is a measure of the induction of the magnet. The current in the second coil is opposed in direction to the current in the primary after reversal—that is, it is in the direction of the current before reversal. If the changes in magnetisation in the iron produce a current in the coil which is connected to the galvanometer, it is clear that they will also tend to produce a current in the coil which in like manner surrounds the magnet, and which has been connected to the battery. This current is what used to be known as the extra current. It continues for a short time the current in the battery coil in the direction which it had before reversal, and retards its change to the opposite direction. If the battery which gives the current is a battery of many cells, and its extra E.M.F. is taken up by ordinary non-inductive resistances, there will be an E.M.F. in the battery to overcome these currents produced by the change of magnetisation, and the reversal of the current will be effected speedily. If, however, the number of cells is only sufficient to produce a current through the resistances of the electromagnet, the change will be much slower.

I have here an ordinary Westinghouse transformer which has an electromagnet in the ring form, but the iron of its core is divided into thin plates for the purpose of preventing currents in the iron itself. There is upon it a primary coil connected to the battery and a secondary coil connected to the galvanometer. I will first show you the effect produced when a battery current is produced by a battery

of one cell giving about two volts. You observe that the current induced in the secondary coil takes a somewhat long time for its production and for its subsequent diminution—about 15 seconds. I will now connect it to a battery of high E.M.F. or rather to the mains which supply current to the building, and we will repeat the experiment with the same current in the primary coil. Observe the difference. The current increases more rapidly and dies away very much more quickly, taking in all about three seconds instead of 15 seconds, as before. I should perhaps here state that the sensibility of the galvanometer has been altered between the two experiments, so as to give a deflection of convenient size to be observed upon the screen.

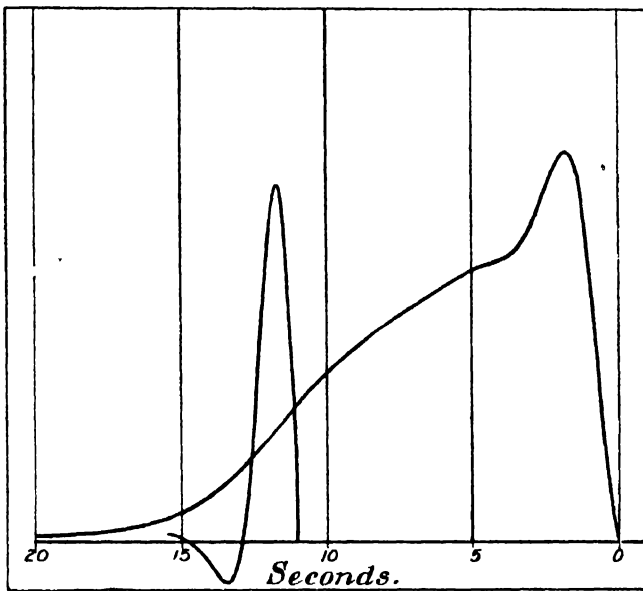
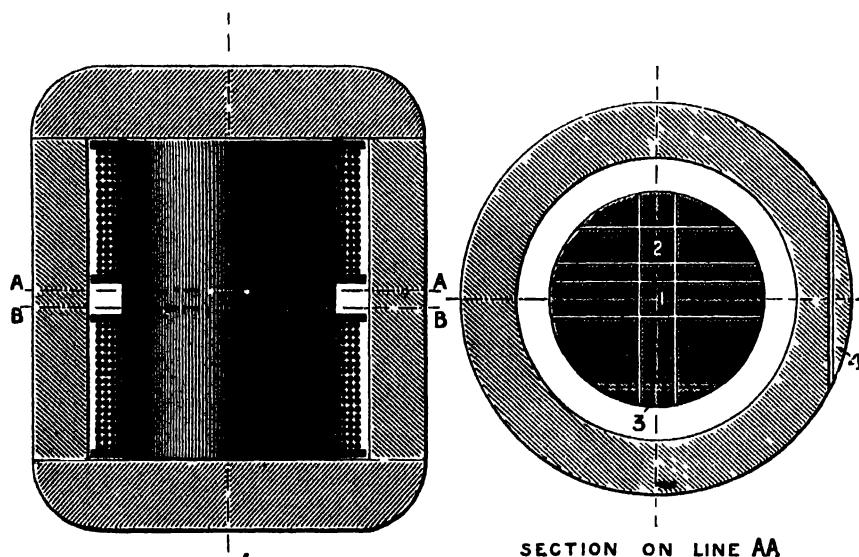


FIG. 1.

In the first case it is very much more sensitive than in the second. Exactly the same experiment can be shown to you in another form. These galvanometer deflections can be exhibited in the form of curves, in which the abscissæ represent times and the ordinates represent the deflections. The curve which I am showing you (Fig. 1) has actually been taken as a photograph, the plate being moved across the field with uniform velocity. On the same plate you see the curves which have been obtained with a larger E.M.F. and with a smaller one. The times at which the reversal occurred in each case are marked by the sudden change at the beginning of the curve. You see exactly the same thing as you saw upon the screen: the big battery makes the changes occur rapidly, the small battery allows them to occur

more slowly. The dip of the curve for the large battery beyond the axes of the ordinates is not a real part of the phenomena. It is caused by the momentum of the moving part of the galvanometer.

In these experiments the iron has been divided in order to get rid of the particular effect about which I wish to speak this evening. Iron, as everybody knows, is a good conductor of electricity, not so good as copper, but still much better than any other substance than the metals. In a magnet, then, with a solid iron core, the outer portions of the iron are in a similar position to the copper coils surrounding the iron. On reversing the magnetising current, currents will be induced in the iron, and these currents will delay the changes



*Scale 1 in to 1 foot.*

FIG. 2.

of magnetic induction within them, and they will delay them the more the deeper in the iron is the point under consideration. I have in the room below a large magnet, which has been expressly constructed for the purpose of investigating the changes of induction which occur at different depths in the iron when the magnetising current is reversed. It is shown diagrammatically in Fig. 2. One view represents the vertical section of the magnet, the other the horizontal section. The magnet consists of a central cylinder of iron surrounded by an annulus with a large slab of iron at each end for the purpose of completing the magnetic circuit. Between the central cylinder and the annulus are placed the magnetising copper coils. In

the solid cylinder holes are drilled at right angles to each other, and these holes meet within the iron. A strand of copper wire has been drawn in through the holes, and has subsequently been connected up so that the coils of the strand are in series with each other. In this way we have a coil surrounding a portion of the iron at 3, at 2, and at 1, of approximately the same area. If these coils then are connected to a galvanometer they will measure the changes of induction which occur in the areas 3, 2 and 1 enclosed, and we shall be able to show you the times at which the currents in the iron permit the effect of reversal in the copper coils to be manifest. We have, in addition, a coil, marked 4, in the outer magnet. Now, when the currents in

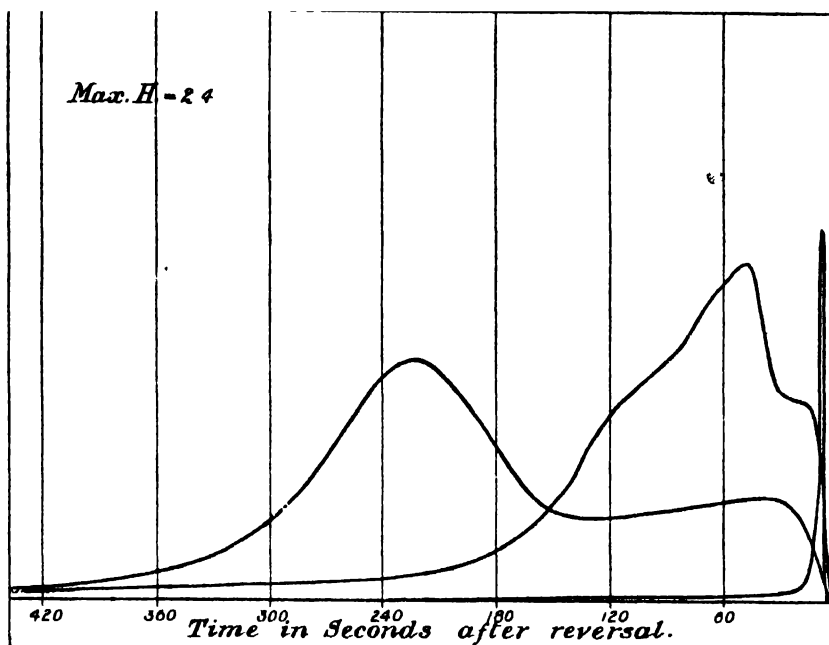


FIG. 3.

the large copper coils are reversed, the magnetism or the induction in the coils 1, 2, 3 and 4 will also be reversed, but more slowly, and currents will be induced in these coils. I propose to show you the way in which these currents occur and are modified by the currents in the iron. We shall see that the changes in induction induce currents in the iron, and that these in their turn delay the changes in induction.

In order to give you a better idea of what is happening, we have provided three galvanometers. The galvanometers throw their images upon the screen at different altitudes. Now, what I want you to observe is this—that the disturbance of induction in the outermost

coil occurs immediately upon reversal of the current, and it occurs after some time in No. 2 coil, and after a much longer time in No. 1 coil, and also to observe the difference of the effects according as the magnetising current is large or small. In all these experiments a considerable battery power is used, and its excess of E.M.F. is taken up with non-inductive resistances consisting of lamps; that is in order to confine the effects to the currents in the iron, and to get rid as far as possible of the effects of induction in the copper coils themselves. We will begin with a somewhat low force, 2.4. You notice that, as shown in Fig. 3, the galvanometer of No. 3 coil is promptly deflected, and as promptly returns to zero, because there is no depth of iron outside it in which currents can circulate. No. 2 coil soon moves off

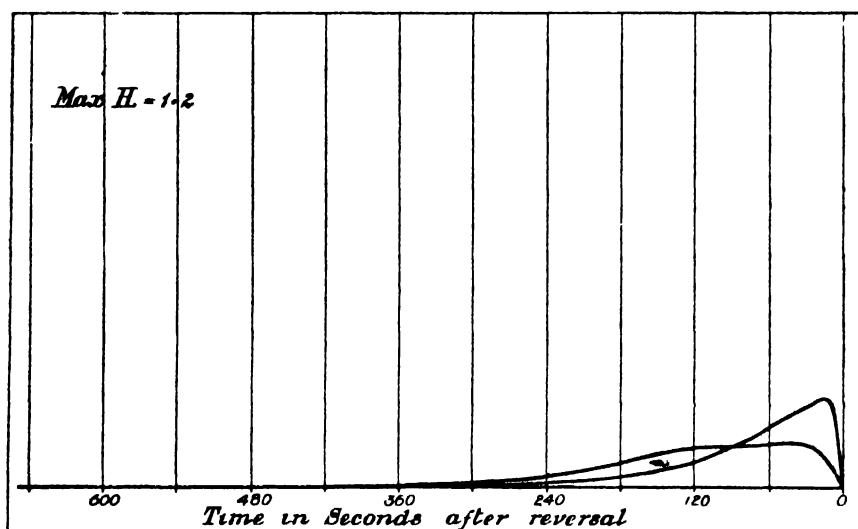


FIG 4

to a substantial deflection, and presently, after 50 seconds have elapsed, it moves off to a greater deflection. The behaviour of No. 1 coil is not dissimilar, but it takes much longer. As in the case of the transformer, the same thing can be shown as a curve (Fig. 3). Observe the relation of currents in the exploring coils to time. No. 3 coil rises to a maximum, and at once ceases. No. 2 goes off fairly rapidly, dwells for a time, and then rises to a full maximum at about 50 seconds; whilst No. 1 coil goes through the same events, but more slowly, as is shown by the increased abscissæ taking four minutes to reach its maximum. Here is a curve (Fig. 4) for a lower force, 1.2, half of the last: notice how great the difference—the maximum is reached much more speedily and the currents drop more slowly. Take a somewhat higher force, 6.0, to see its effect on the galvano-



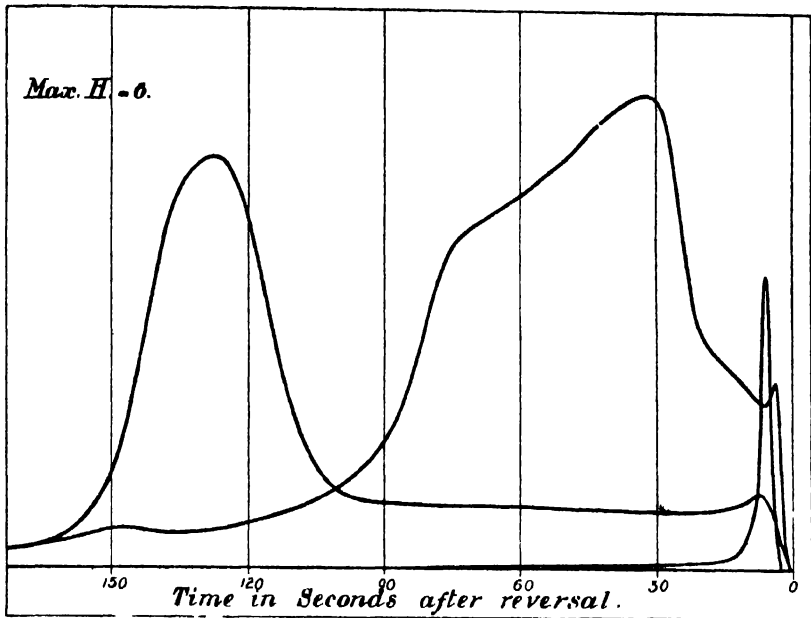


FIG. 5.

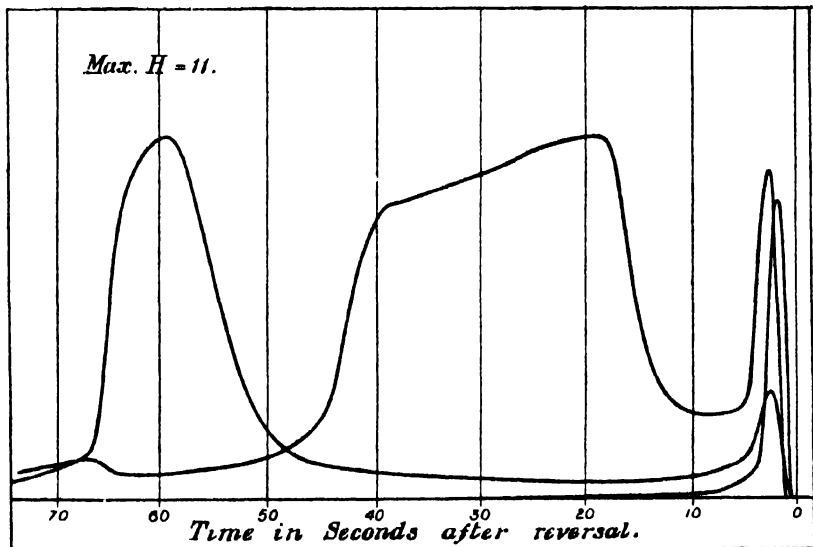


FIG. 6.

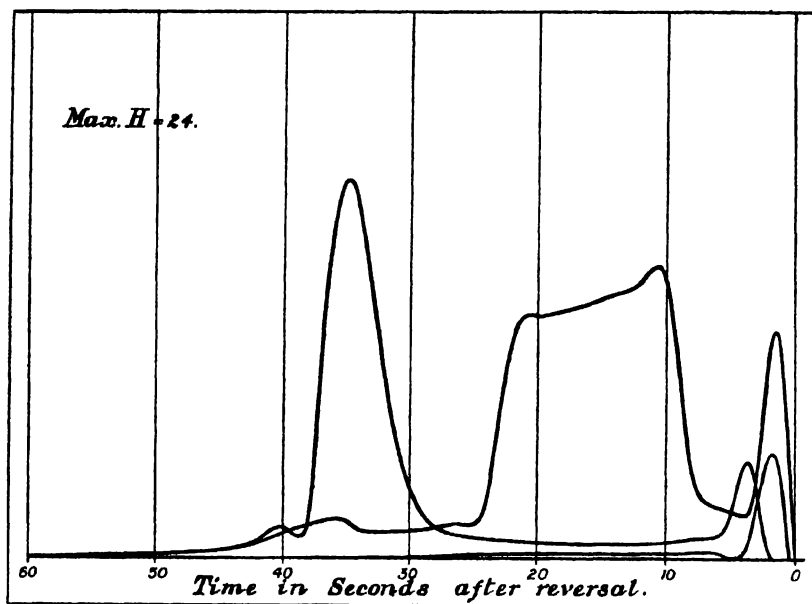


FIG. 7.

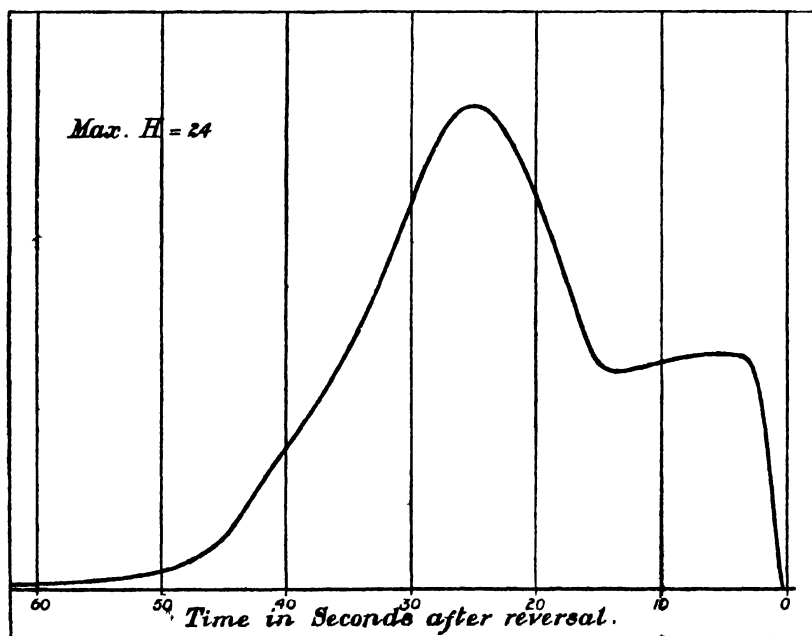


FIG. 8.

meters. The sensibility of No. 3 galvanometer is much less than that of Nos. 2 and 1, which are about the same. You see a new phenomenon develop: there is a first maximum in Nos. 2 and 1 coils, followed by a dwell and then a rise to a second maximum. Look at the corresponding curves (Fig. 5). The maxima are occurring earlier than with the force of 2.4—that for No. 2 coil at 30 seconds instead of 50 seconds, and of No. 1, the centre coil, at 130 seconds instead of 240. Again take a higher force, this time 11 (Fig. 6): see how marked the first rise has become. On the curve notice that the maximum of No. 1 coil is as great as No. 2—with lower forces it has always been less. The highest force I shall show you is 24 (Fig. 7), and it is really the prettiest. The first maxima occur markedly at once. No. 2 coil comes down to a small value, then rises to a substantial maximum in

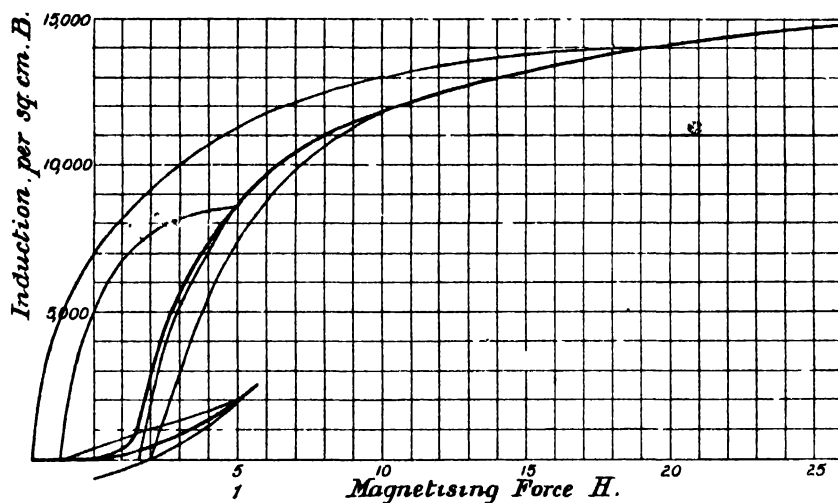


FIG. 9.

about 10 seconds, remains near its maximum value for 10 seconds, and passes away. Whilst No. 2 has been at its maximum, No. 1 has been quietly remaining undisturbed, and only when No. 2 has quite finished does No. 1 suddenly move off to a maximum greater than No. 2 at 35 seconds and then very suddenly disappear. We must not leave this part of the subject without showing one experiment with No. 4 coil, which is placed in the annular part of the magnet. We will use the maximum force (24). The general character of the changes is the same as before, but in this case the magnetism changes at once in the interior of the ring, and later at parts nearer to the outside. You see the same thing from the curve, Fig. 8.

I am afraid I can only give you a very general explanation of the peculiarities of these curves. I will first of all throw upon the screen a diagram (Fig. 9) showing what are known as cyclic curves of

magnetisation. The vertical ordinates represent the induction, and the horizontal abscissæ the magnetising force. You will observe that when the magnetising force is small the ratio of induction to magnetising force is also small; that as the magnetising force increases this ratio is also increased for a time, but that, finally, it again becomes small. You will observe, further, that when the magnetising force is great the curve of magnetisation begins with a small rate of change, which then becomes large, and which again becomes finally small. Now the slowness of changes of induction depends upon the largeness of the ratio of induction to magnetising force, and, therefore, we should expect that when this ratio is small the changes would occur with comparative rapidity, and that when it is large they would occur more slowly. This is exactly what we

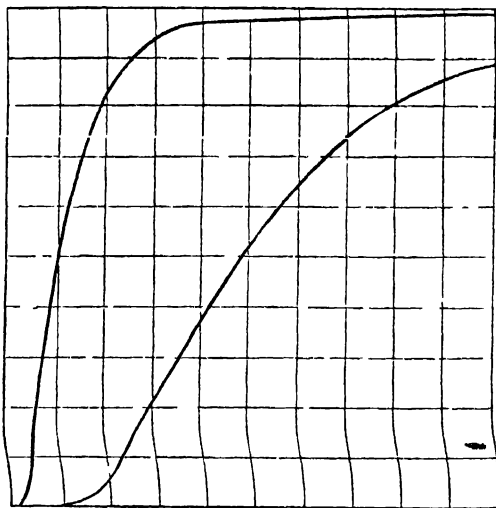


FIG. 10.

have seen. With a small force the change shows itself almost at once; with an intermediate force the principal maximum occurs much later, and with a large force it is continuously being accelerated as the force becomes larger. The first rise that occurs with a large force is due to the early part of the curve of induction, where changes of induction are comparatively small, and where consequently the effects rush in rapidly. This first maximum is followed by a time of comparative quiescence, which is again succeeded by a second and large maximum caused by the full effect of the great changes of induction which have occurred, but which have passed in comparatively slowly; and then, finally, the change drops away to zero quite suddenly, owing to the great rate at which the last part of the change has rushed in and pressed upon the parts which had occurred before it.

These phenomena of the times at which changes of induction occur in the cores of electromagnets have a close analogy to the retardation of signals upon cables. Many of you know if a battery is connected to a submarine telegraph cable, and if its connection is suddenly reversed, that the current throughout that cable is not reversed at once, but that it takes an appreciable time before the effect is perceptible at the far end of the cable. This retardation of the signal, as it is called, rapidly increases with the length of the cable. I will first of all show you two curves of arrival, as they are called, of the current in a cable (Fig. 10). In these curves the ordinates represent the currents, and the abscissæ represent the times. You will observe that there are two curves shown; one is

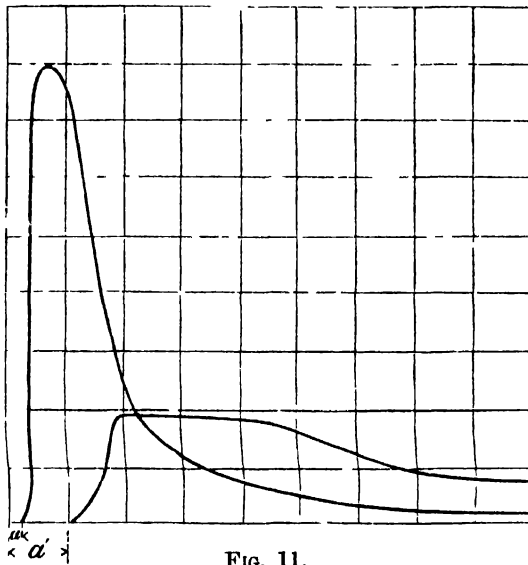


FIG. 11.

for a length of cable double the other, the longer cable giving the slower times. Now all that the curves in this form are capable of showing you is that with the longer cable the events are more retarded. In order to make them really comparable with the magnetic experiments, we must show a curve in which the abscissæ represent, not the currents, but the rate of change of current. We have such a curve here (Fig. 11). In this case the abscissæ represent the rate at which the current is changed, and the ordinates represent the time. This curve has a remarkable similarity to the magnetic curve with the lowest current. The similarity of the curves, as shown in Fig. 12, is quite sufficient to suggest an analogy between the phenomena which actually exist. I can show you the same thing in another way. There are in the library one of Lord

Kelvin's syphon recorders, made by Messrs. Muirhead & Co., which will be at work after the lecture, and also an artificial cable, which has also been made by Messrs. Muirhead & Co. Curves can be obtained from the end and middle of the cable.

Magnetic experiments such as these have really a wide application. They are not restricted to the particular size of the cylinder upon which the experiment is made. If the cylinder is larger or smaller similar magnetic events will happen, but they will happen at times shorter or longer in direct proportion to the square of the linear dimensions of the cylinders—that is, to the areas of the cylinders. Hence we may boldly infer what will happen in magnet cores of dimensions too small to experiment upon or so large that they would be very costly to make. For example, suppose a core is

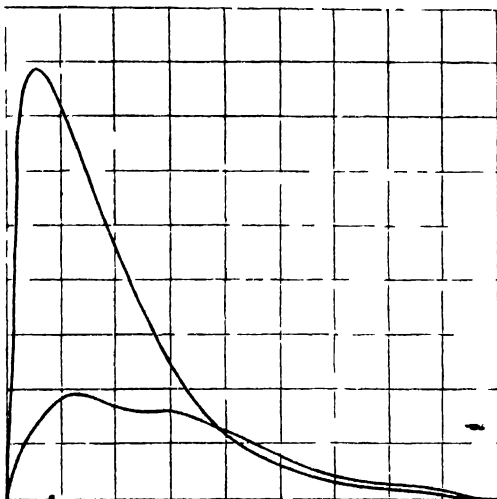


FIG 12.

made up of wires  $\frac{1}{100}$  of an inch in diameter, we may expect that everything will go on with the same currents around the core but 1,440,000 times as fast; with a force of 24, everything will be over at the centre of the core if the current is instantaneously reversed in  $\frac{1}{40000}$  of a second.

One very practical application of these results is to the cores of transformers, but in this case we have not the sudden reversal of the magnetising current, but the current continuously varies from positive to negative and from negative to positive. It is not difficult to imitate these conditions on the slow scale which is suitable for our large magnet, but if I were to show you an experiment at an appropriate rate on the magnet which we used for reversals, I fear you would find the results intolerably tedious. It would take some minutes to get through a single reversal. I will, therefore, use

a smaller magnet, 4 inches instead of 12 inches diameter, which will get through the business nine times as speedily. I will first of all throw upon the screen a diagram (Fig. 13) which shows the method of the experiment. Here is the source of electricity, marked "dynamo"; here are resistances for regulating the current, marked "rheostat"; and here is a liquid reverser intended to reverse the direction of the current by continuous steps. It consists of two copper plates, to which the current is taken, and of two moving plates revolving between these plates, which plates are

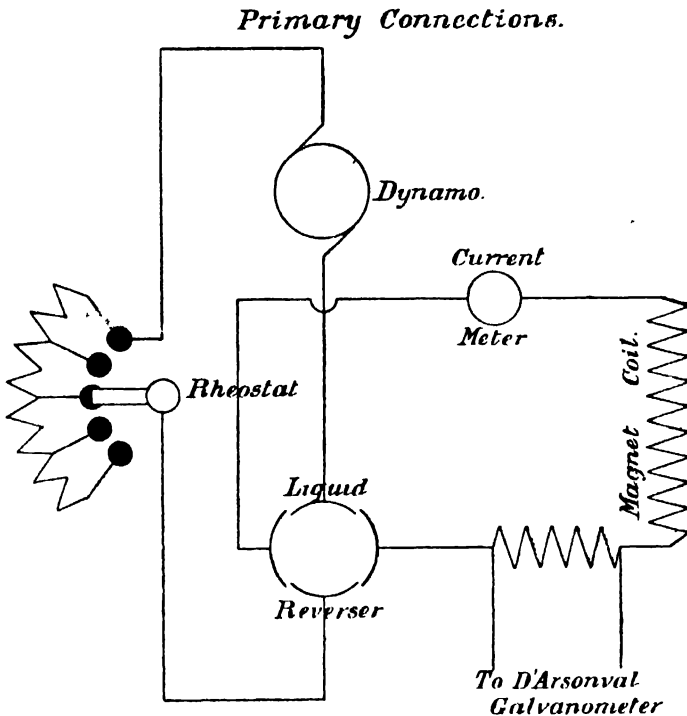


FIG. 13.

connected to the electromagnet. As the moving plates revolve, the current will diminish from a maximum in one direction to nothing, and will then increase to a maximum in the opposite direction. The electromagnet is marked "magnet," the secondary coils of which are not indicated in the diagram. Only one coil in the centre of the magnet is connected. We will now turn the reverser faster. You see, the variations of inductions diminish greatly—indeed, the induction at the centre of the core is but little affected by the changing current in the magnetising coils (change leads). We might have tried three coils instead of one, but the experiment is a little

confusing. If we had done so, we should have seen that the diminution of disturbance with increase of speed was less with No. 2 coil than with No. 1 coil, and that it had disappeared entirely with No. 3 coil, and we should further see that the current in No. 2 coil lay behind No. 3, and in No. 1 behind No. 2. I can show you the contrast more effectively with the Westinghouse transformer with a divided core. This is now connected to the lowest galvanometer. We turn slowly: notice the deflection. We turn faster: you see the deflection is increased instead of diminished, as it was with the central coil of the solid electromagnet. Experiments such as these are at once applicable to transformer cores and the cores of dynamo machines. They show that in practice manufacturers have divided the iron about enough, and not too much.

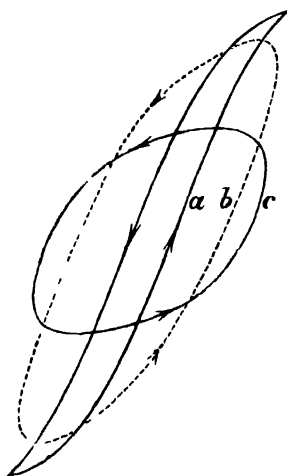


FIG. 14.—MAGNETIC CURVE-TRACER CURVES FOR SOFT IRON BARS.

- (a) Cycle performed slowly.
- (b) Period of cycle 3 seconds.
- (c) Period of cycle 0.43 second.

I have here on the table an instrument designed by Prof. Ewing for the purpose of describing the curves which express the relation between induction and magnetising force in iron. It served in Prof. Ewing's hands to illustrate the point which we have been discussing. I will throw upon the screen curves taken from Prof. Ewing's paper read before the Royal Society. The curves have been taken from solid samples of iron—I mean iron which is continuous, and not divided for the purpose of annulling the current in it. In the first curve (a, Fig. 14) we have the result in which the cycle has been passed through very slowly, and is the true curve of magnetisation. We have b, where the cycle has passed through in three seconds—and you will observe that the amplitude of the



induction has diminished, and we have, lastly, the curve *c*, where the period of the cycle is four-tenths of a second, and you will observe that it has much more diminished. This is owing to the fact that changes of induction in the centre of the iron hardly exist, and that therefore the total effect is materially diminished. The diagram (Fig. 15) is for steel. We have *a* the natural curve of induction; *b* the case when the period of the cycle is three seconds; and *c* when the period is six-tenths of a second. Curves such as these, of course, very readily give you the average effects upon the whole mass of the iron. The curves which I have shown you give the particular effects of different parts of the mass.

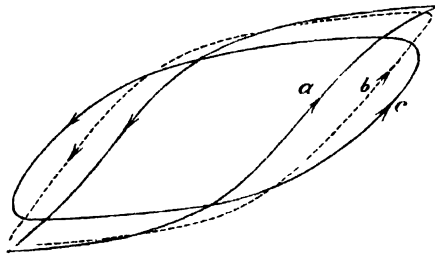


FIG. 15.—MAGNETIC CURVE-TRACER CURVES OF STEEL.

- (a) Cycle performed slowly.
- (b) Period of cycle 3 seconds.
- (c) Period of cycle 0.6 second.

In conclusion, let us indulge in a little wild speculation, not because it is probable that it is in any sense true, but because it is interesting. Suppose a magnet were made exactly like the one on which we experimented, but of the size of the earth, and that some mighty electrician generated such a current in its copper coils as would give a magnetising force of 2.5, and then reversed it, it would take some thousands of millions of years before the rate of disturbance at the centre attained its maximum value. The speculation I suggest is this: is it not conceivable that the magnetism of the earth may be due to currents in its material sustained by its changing induction but slowly dying away?

[J. H.]

Friday, May 24, 1895.

WILLIAM CROOKES, Esq. F.R.S. Vice-President, in the Chair.

J. VIRIAMU JONES, Esq. M.A. F.R.S. *M.R.I.*

*The Absolute Measurement of Electrical Resistance.*

IF we take a conductor, such as this piece of copper wire, and make it part of a conducting circuit in which an electric current is flowing, we find that the electromotive-force between its extremities is proportional to the strength of the current passing through it, so long as it remains in the same physical condition. If either the electromotive-force or the current strength varies, the other of these quantities varies in like proportion. Their ratio is constant, i.e. it has a value independent of them, and depending only on the dimensions of the conductor, the nature of the material of which it is made, the state of aggregation of its parts, and its temperature.

This ratio is called the electrical resistance of the conductor, and so defined it corresponds to a real physical quantity—it is a physical property of the conductor.

Probably no physical measurement can be made with greater accuracy than the comparison of electrical resistances. Such comparisons are daily made in many parts of the world, and it is clearly desirable, in order that the results obtained by one man may have meaning for others, that all should make their reckoning in terms of the same unit. Accordingly, the scientific world has given great attention to the definition of such a unit, and much international negotiation has taken place with a view of securing world-wide agreement on this point.

The most obvious method of procedure is to fix upon some convenient conductor as a standard, and to call its electrical resistance the unit of electrical resistance, other electrical resistances being then expressed as so many times or such and such a fraction of the resistance of this standard conductor.

The disadvantage of this method of defining the unit is that the resistance of such a standard may change from time to time through alterations of its physical condition. The most notable change is consequent on change of temperature; this may, however, be allowed for by defining the unit as the electrical resistance of the standard conductor when it is at a specified temperature. But the other changes of physical condition, changes in the state of aggregation of the parts of the conductor, effects of strain, alterations of molecular structure, &c., are more serious, because they are unknown, and we cannot in our definition provide against unknown possible changes.

We run the risk, therefore, of having a varying unit, and the prime requisite of a unit is constancy.

In England the Government has taken this risk, and committed itself—at least for the present—to this method of specifying the unit of electrical resistance.

The denomination of the standard of electrical resistance contained in the recent Order in Council regarding Standards for Electrical Measurements is as follows :—

“ A standard of electrical resistance denominated one ohm, being the resistance between the copper terminals of the instrument marked ‘ Board of Trade Ohm Standard Verified, 1894 ’ to the passage of an unvarying electrical current, when the coil of insulated wire forming part of the aforesaid instrument and connected to the aforesaid terminals is in all parts at a temperature of  $15.4^{\circ}\text{C}$ . ”

This is a plain adoption of an arbitrary standard. The resistance of a certain piece of wire wound into a coil is made the legal unit, and if this resistance varies our legal unit varies with it.

It is true that in constructing the standard, efforts have been made to ensure that its resistance shall be equal to 1000 million times the absolute unit of which I shall presently speak—that the standard was in construction based upon the ohm ; but there is no provision in the Order in Council for revision if the standard varies, nor indeed has the Government any means at its disposal of directly measuring its standard in absolute measure.

I seem to be finding fault. That is not my intention. I am only advocating progress. The time is ripe for the adoption of the absolute unit, not merely nominally but really, for the frank acceptance of the absolute unit itself as the ultimate legal standard.

Now, what is an absolute unit ? The following considerations affecting physical measurements generally will, I hope, serve to make this clear.

In order to specify the magnitude of any physical quantity, we bring it into relation with a standard magnitude of the same kind, called the unit, and say that it is so many times or such and such a fraction of this unit. Thus we speak of a length of 6 centimetres, a mass of 50 grammes, a time of 20 seconds, an electric current of 10 amperes, an electrical resistance of 30 ohms, and so on.

For the purpose of this specification any convenient magnitude of the physical quantity to be measured may be taken as the unit.

But science has to deal with many kinds of quantities, and when we consider the various units as constituting a system, our arbitrary choice is limited by two conditions of fundamental importance :—

1. The units must be chosen so as to simplify as far as may be the statement of the quantitative relations existing between various kinds of quantities.

Physical science, in so far as it is quantitative—may we not say in so far as it is perfected ?—tells us of relations of interdependence between many different kinds of quantities. Thus, in geometry we

have such relations between lengths, areas, and volumes; in kinematics, relations between these mathematical quantities and times, velocities, and accelerations; in dynamics, between these mathematical and kinematical quantities and mass, momentum, force, work, and energy; in electrical and magnetic science, relations between the foregoing quantities and electrical and magnetic magnitudes; and so on.

These relations of interdependence are expressed in equations, and it is of obvious advantage, both for ease of arithmetical calculation and clearness of thought, to rid these equations as far as possible of superfluous arithmetical constants. This may be done by a judicious selection of units, by making the units of the quantities that appear most complex in their relations of interdependence depend upon the units of the quantities that appear in their nature to be simplest. A unit of any quantity so defined with reference to the units of quantities apparently simpler is called a derived unit with reference to the latter units as fundamental.

Thus, the attraction of the earth on a mass of 1 lb.—the weight of 1 lb.—is an arbitrary unit of force. The force that, acting on a mass of 1 lb., increases its velocity by 1 ft. per second every second is a derived unit of force with reference to the units of length, time, and mass, as fundamental. It is usually called the absolute unit of force on the pound-foot-second system; and by the very nature of its definition it gets rid of an arbitrary constant in the relation between force, mass, and acceleration. If the arbitrary unit of force mentioned above—the gravitation unit—is used instead of the absolute unit, our dynamical equations are uselessly complicated by the introduction of “*g*,” the acceleration of gravity at the particular point of the earth’s surface at which we happen to be, and at the particular time when our measurements happen to be made.

2. The second condition of prime importance to a scientific system of units is that the units of all quantities should be invariable, unaffected by conditions of time and place, and independent of the properties of particular bodies, i. e. they should be *absolute*. The word “absolute” is in philosophy opposed to “conditioned.” When it is applied to a unit in science the implication should be that the unit is the same at all times and in all places, and that it is unconditioned by the properties of any specified body or bodies, i. e. that its magnitude is brought into relation with and depends upon only the most permanent phenomena of the universe.

The modern system of absolute units goes far to fulfil the first of our two requirements; it only very partially fulfils the second.

When we speak of an absolute unit at present we mean a unit the magnitude of which depends on nothing else than the units of length, time, and mass, and the properties of the ether. The latter may be regarded as universal enough, but the units of length, mass, and time are arbitrary standards. The metre depends on the properties of a particular bar, the gramme on the properties of a particular

piece of platinum, the mean solar second on particular bodies of the solar system.

The time will come when we shall be able to take another step forward, when we shall be in a position to make the unit of mass a derived unit with reference to the units of length and time as fundamental through that universal uniformity of nature—the law of gravitation; the units of length and time being based on some more permanent phenomena of the universe (the properties of the sodium molecule have, for instance, been suggested) than the length of a particular bar, and the motion of the earth on which we live.

But we cannot take this step forward yet. We do not know either the gravitation constant or the velocity of light with sufficient accuracy to enable us to define by means of them units precise enough to meet the needs of practical life. Yet the fact that if we knew these constants we could make an important advance in the realisation of a system of absolute measurement is an excellent reason why the masters of physical measurement should apply themselves to their precise determination.

The use of a derived unit of mass would have this among other advantages, it would rationalise the dimensions of the electric and magnetic units in length and time, and relegate the irrationality to the ether, to which I believe it properly belongs.

We must, however, for the present be content with a less complete reference to the most permanent phenomena of the universe, and by absolute measurement to-night I mean measurement in terms of a unit derived with reference to the unit of length, time, and mass as fundamental—i. e. a unit conditioned only by these units, and the properties of the ether.

There is such a unit of electrical resistance—a unit derived with reference to the units of length and time as fundamental—the magnitude of which is independent of the properties of any particular conductor, any particular coil of wire, and is conditioned only by the units of length and time, and the properties of the ether.

It may, indeed, at first sight seem astonishing that a quantity so different can be expressed in terms of a length and a time. Yet so it is. We can, in virtue of experiments made 63 years ago in this Institution by Michael Faraday, measure the resistance of a conductor with the help of no other standardising instruments than a tape or measuring machine and a clock. An electrical resistance is always proportional to a certain velocity, and if the magnetic permeability of the ether be taken to be unity, the number expressing the electrical resistance will be the same as the number expressing that velocity; the unit of electrical resistance may then be taken to be the resistance corresponding to unit velocity—i. e. on the C.G.S. system a velocity of 1 c. per second, and any electrical resistance may be conveniently expressed in terms of this unit as so many centimetres per second.

[You will observe that I do not say electrical resistance is a

velocity. That would be to neglect the unknown dimensions of the magnetic permeability of the ether.]

If the unit of length, the unit of time, and the properties of the ether remain constant, this unit of electrical resistance remains constant. It is unconditioned by the properties of any material, by position in space, or point of time, and so far deserves the name of the absolute unit of electrical resistance.

How comes it that resistance can be so measured? The answer to this question is best found in a description of some one of the methods by which the measurement of an electrical resistance in terms of the absolute unit can be experimentally made.

And I proceed, therefore, to a description of the method which may fairly be described as the simplest, and which, in my opinion, having regard to the magnificent possibilities of mechanical engineering operations in this country, is undoubtedly capable of being made the most accurate of all the methods that have been proposed since the British Association Committee, more than thirty years ago, propounded the theoretical definition of the absolute unit.

The method is due to Lorenz; and Lord Rayleigh, at the conclusion of his masterly determinations of the value of the B.A. coils in absolute measure, expressed himself in regard to it as follows:—

“On the whole, I am of opinion that if it is desirable at the present time to construct apparatus on the most favourable scale so as to reach the highest attainable accuracy, the modification of Lorenz’s method last described is the one that offers the best prospect of success.”

The Paper from which I quote contained a comparison of the various methods of measuring resistance in absolute units, and an invitation to others to join in the work. It was the starting-point of my own researches in the matter, which have extended over some years, and I gladly take the opportunity of thanking Lord Rayleigh for this source of inspiration.

Faraday discovered that, if a conductor is made to move in a magnetic field so as to cut across the flux of magnetic induction, the conductor becomes the seat of electromotive-force, and that the electromotive-force so developed is proportional to the rate at which the induction flux is traversed.

If, for instance, we take a metal disc and make it rotate about a horizontal axis  $n$  times a second, any radius of the disc cuts through the earth’s induction flux, unless the plane of the disc is in the magnetic meridian. There will, therefore, be electromotive-force between the centre and circumference of the disc. Further, since a radius traverses the whole area of the disc  $n$  times in a second, the rate at which the induction flux is being traversed by any radius is  $n I$ , where  $I$  is the total flux of induction through the disc area. But the electromotive-force is proportional to this rate. Therefore we have (with a proper choice of the unit of electromotive-force)

$$E_1 = n I,$$

where  $E_1$  is the electromotive-force between the centre and circumference.

But we may, instead of using the earth's field, obtain a magnetic field by means of an electric current. The disc may be made to spin in the magnetic field due to a current in a coil of wire placed so as to be co-axial with it, and so that its middle plane coincides with the plane of the disc. In this case the magnetic field is symmetrical with regard to the common axis, and we have once more

$$E_1 = I n,$$

where  $E_1$  is the electromotive-force between the centre and circumference consequent on the rotation of the disc at the rate of  $n$  turns per second in the induction flux due to the current in the coil,  $I$  being the total amount of that flux which passes through the disc circumference.

Now the current strength in a given circuit is by definition proportional to the intensity of the magnetic field produced by it at any point, and hence the magnetic induction through the disc due to the current in the coil is proportional to the strength of that current.

It follows that the magnetic induction through the disc is made up of two factors, viz. the current in the coil and the magnetic induction that would pass through the disc if unit current passed through the coil. The latter factor is called the coefficient of mutual induction of the coil and disc circumference, and its value depends only on their dimensions and relative positions, and the magnetic permeability of the medium in which they are placed. If the latter quantity is taken to be unity, the coefficient of mutual induction is expressible as a length, and it may be calculated from observations involving nothing else than measurement of the radius and breadth of the coil and the radius of the disc. Let this coefficient of mutual induction be denoted by  $M$ , and the current in the coil by  $\gamma_1$ ; then

$$I = M \gamma_1;$$

and finally,

$$E_1 = n I = n M \gamma_1;$$

or

$$\frac{E_1}{\gamma_1} = n M.$$

We have therefore two ways of expressing the ratio of an electromotive-force to a current.

By definition (Ohm's law) this ratio is given to us as electrical resistance, or

$$\frac{E}{\gamma} = R.$$

By the experiment of the disc and coil it is given to us as the product of their coefficient of mutual induction and the rate of rotation of the disc (number of turns per second).

And if the ratio for the disc and coil is made the same as the ratio for the resistance (which it is possible in practice to arrange) we have

$$R = M n.$$

If the magnetic permeability of the medium be taken as unity,  $M$  is a length expressible as so many centimetres, and the number of turns per second is the reciprocal of the time of one revolution of the disc. Hence the resistance may be expressed as a velocity of  $M$  centimetres in the time of one revolution of the disc. This makes it clear that, with the assumption in regard to the permeability of the ether (or air) that we have made above, any electrical resistance is expressible as a velocity of so many centimetres per second.

We can in practice make the ratio for disc and coil the same as the ratio for the resistance by first sending the same current through coil and resistance and then varying the speed of the disc so as to make the electromotive-force between its centre and circumference equal to the electromotive-force between the resistance terminals.

How can we test the equality of the two electromotive-forces? If two equal electromotive-forces act in opposite directions in the same circuit the result is electrical equilibrium. We have, therefore, to proceed as follows:—Place the two electromotive-forces in the same circuit so as to act in opposite directions; then if they are the only electromotive-forces in the circuit, there will be an electric current round the circuit due to their difference if they are unequal, and no electric current at all if they are equal. The presence or absence of a current may be tested by the inclusion of a galvanometer in the circuit.

In practice it is not quite so simple as this, because it is impossible to make the two electromotive-forces in question the only electromotive-forces in the circuit. There will be others, and especially thermo-electric forces, always present. But this difficulty may be met thus:—Reverse the two electromotive-forces we are comparing; if the current in the circuit is not changed by this reversal of both, they must be equal—i. e. if the reading of the galvanometer be the same, whether the balancing electromotive-forces are in one direction or the other, these electromotive-forces are equal. The reversal of both our electromotive-forces may be readily effected in practice by simply reversing the direction of the current through the coil and resistance.

To measure our resistance in absolute measure we have then—

1. To make the coil (SSS, Fig. 1), and the resistance (XY) parts of the same main circuit (BCFSSSPXYQCB), and to pass through this circuit an electric current. A commutator (O) is to be inserted in the circuit in order that the direction of the current may be changed at will.

2. To insert the electromotive-force between the centre and circumference of the disc (DDD) acting in one direction and the



electromotive-force between the extremities of the resistance acting in the opposite direction in a second circuit (X M O G Y X) containing a sufficiently sensitive galvanometer G. Brush contacts must be made at O and M.

3. To vary the rate of rotation of the disc until the reading of the galvanometer is the same whether the current in the main circuit through coil and resistance is being sent by the commutator in one direction or the other.

4. To measure this equilibrium rate of rotation.

5. To multiply this equilibrium rate of rotation (number of turns per second) by the coefficient of mutual induction of the coil and disc, which is calculated once for all from their measured dimensions. This product gives the resistance to be measured in absolute units.

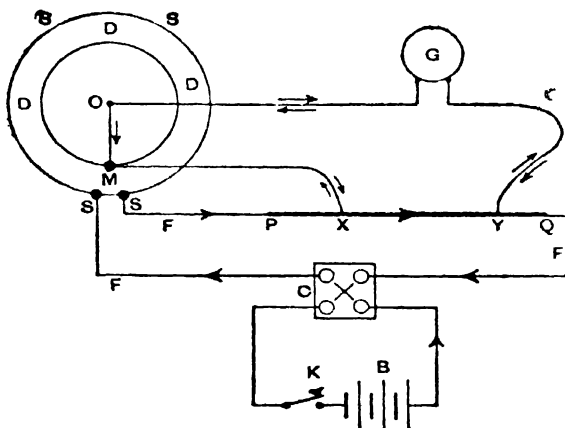


FIG. 1.

[The necessary arrangements were here experimentally shown by the lecturer.]

I trust that I have said sufficient to make clear to you the general theory of the measurement of a resistance in absolute measure by this method. It now remains to consider very briefly the practical side of the matter, the difficulties that arise, the way in which they may be met, and the accuracy attainable. The time at my disposal does not allow me to do this at all completely, and I must content myself with touching on a few points of special importance and interest.

The first great desideratum is that we should have the rate of rotation of the disc well under control, that it should run as uniformly as possible, and that its rate should be capable of sufficiently exact determination.

The disc, axle, and bearings in my apparatus at Cardiff are of phosphor bronze (Figs. 2 and 3). The disc is insulated from the

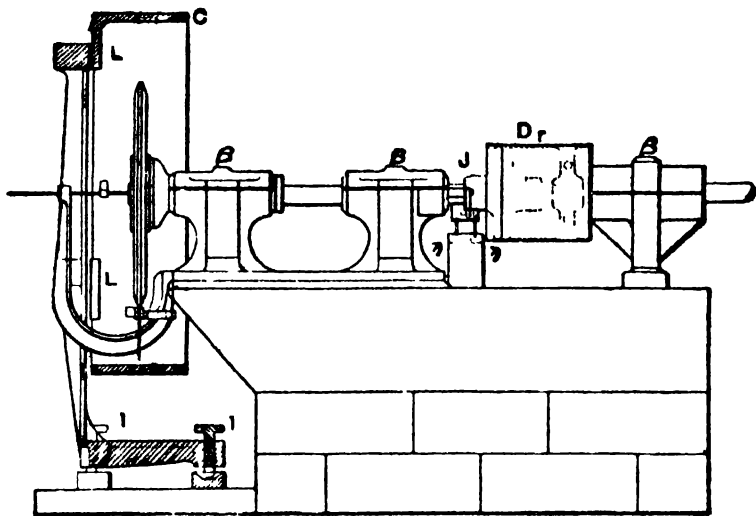


FIG. 2.—Disc and Coil. Elevation and part section on line A B.  
Dr = Drum with rows of teeth on it.

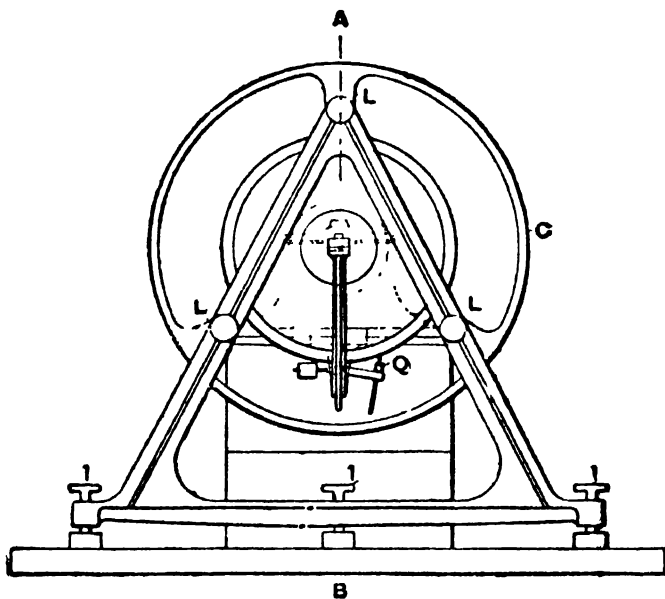


FIG. 3.—Disc and Coil. End View.

axle by well-paraffined ebonite (Fig. 4), otherwise there would be short-circuiting of part of its radius through the bearings and bed.

It was at first intended to drive the disc by rope gearing from the electromotor; but in the course of preliminary experiments, though the rope was 40 yards long, and joining of the ends took place over a length of at least 6 feet, there was a sudden variation in speed, producing a distinct movement of my galvanometer needle, and visible at the tuning fork, of which I shall presently speak, whenever the joint passed over the pulley. The rope gearing was therefore abandoned, and the motor coupled direct. The current driving the

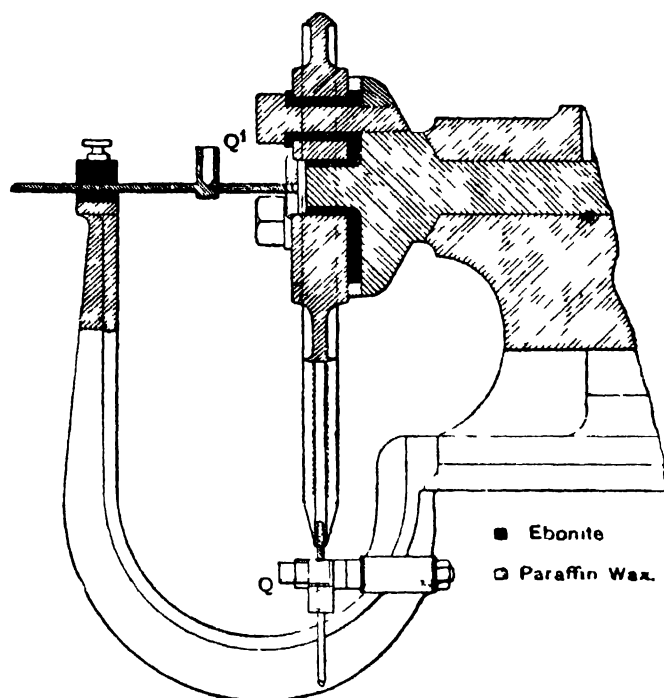


FIG. 4.—Details of Disc Insulation and Brushes. Part Elevation and part Section on lines *ab*, *bc*, *Q* *Q'* = brushes.

motor is supplied from secondary cells. It passes to the motor through resistance coils, and may be increased or diminished by throwing some of these coils into or out of the circuit. It may also be varied continuously through a small range by a slide resistance of platinoid wire after the larger adjustment has been made. A shunt worked by a lever provides means of taking out or putting in a small resistance suddenly, so as to allow the observer controlling the speed while an observation is being taken to counteract small variations of speed due to alteration in the lubrication of the bearings and the friction of the brushes on the commutator of the motor. With these

arrangements we can obtain any required rate of rotation from about 150 to 1500 revolutions per minute.

It is of the greatest importance, if the observations presently to be described are to be made with ease, that the rate of rotation should remain constant for the four or five minutes required, and no effort made to compass such uniformity is thrown away. The design, workmanship, and lubrication of all the bearings require closest attention, and the friction between the brushes and the motor commutator should be reduced to a minimum. In my Cardiff apparatus the axle near the motor bears a heavy fly-wheel.

During a run the observer controlling the rate of rotation requires some test of uniformity, so that he may, if there is a quickening or slowing down, diminish or increase respectively the current through the electromotor so as to recover the initial speed.

To accomplish this it is convenient to refer the rate of rotation by a stroboscopic method to a suitable tuning fork provided with riders and maintained in vibration electrically. Here is such a fork. At the end of each prong there is an aluminium plate with a slit in it parallel to the prong. When the fork is in equilibrium the slits are opposite one another, and an observer can see through both. When the fork is in vibration he can see through the slits only when they are opposite one another. This position occurs twice in every complete vibration. Hence, if the number of vibrations per second is, say, 64, he obtains 128 views per second.

Now let us suppose that he looks through the vibrating slits at a drum or disc coloured black, on which a number of white spaces, which I will call teeth, are painted at equal angular intervals. Here is such a disc attached to the rotating axle; on it there are three circular rows of radial teeth.

When the disc is in rotation, if the rate of rotation is such that in the interval between two views a tooth in any row exactly takes the place of the next, that row will appear, to an observer looking through the fork slits, stationary; if it does not quite do so, the row will appear to be moving backward; if it passes beyond, the row will appear to be moving forward.

These phenomena I can show you by illuminating the rotating disc by intermittent light. The light of this alternating arc varies in intensity at each alternation, passing from maximum to maximum through a minimum that is, however, very far from darkness. Our speed is such as to make the row of fourteen teeth appear almost stationary in this intermittent light. You can see the teeth, but they stand out somewhat faintly. The faintness is due to the fact that we have not complete darkness between the maxima of illumination.

We may do much better with such a tuning fork as I have described. I place its slits in front of this electric lantern, in which there is a continuous arc. The light falling on the disc has passed through the slits of the vibrating fork, and by varying the rate of revolution we obtain perfectly distinct stationary teeth. Let us

suppose that the fork permits the light to pass through it 128 times per second, and that the row on the disc which looks stationary has fourteen teeth on it; then one tooth takes the place of the next in  $\frac{1}{128}$  a second, but this corresponds to  $\frac{1}{14}$  of a revolution, so that one revolution is performed in  $\frac{1}{128} \times 14$  of a second, and the number of turns per second is  $\frac{128}{14} = 9\frac{1}{7}$ .

And generally

$$n = \frac{2P}{Q},$$

where P = the pitch of the fork,

n = the number of turns of the disc per second,

Q = the number of teeth in the stationary row.

We may then make the stationariness of a tooth, as seen by an observer through the slits, a test of the uniformity of the rate of rotation. If a tooth begins to move past a fixed reference wire placed immediately in front of the rotating disc or drum, the observer can at once bring it back by altering the current in the motor.\*

If the tuning fork were itself a reliable time indicator to the degree of accuracy required, we might calculate from its pitch the speed corresponding to stationariness of a given row of teeth. But my experience is that the vibration period of a fork maintained in vibration electrically is not sufficiently constant. If stopped and set going again it may start with a period different from that of its last performance by several parts in 10,000. No previous determination of the pitch of the fork can therefore be relied on to give us the rate of rotation to a hundredth per cent., though once started the fork goes sufficiently uniformly to give us the means of control.

The period of a bowed fork does not vary in this fashion, but it is more troublesome to use for the purpose. The constant bowing required takes too much of the attention of the observer.

Accordingly, it is necessary to measure the rate of rotation during each run while the galvanometer observations are being made. This may be done telegraphically with great accuracy. The rotating disc is by means of an eccentric attached to its axle caused at each revolution to make and break an electric circuit passing through a Bain's electro-chemical telegraph instrument. The standard clock telegraphs seconds to the same instrument. The two records lie side by side on the same tape, and simple counting with careful fractional estimate gives with great precision the mean rate of rotation during the few minutes occupied in taking a set of galvanometer readings.

With this means of measuring the rate of rotation we may, establishing synchronism with a fork in the way I have indicated, find the pitch of the fork.

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\* So far as I know this method of measuring a rate of rotation was first used by Lord Rayleigh. The method used by Macleod and Clarke for measuring the pitch of a tuning fork bears much likeness to it.

Table I. gives a set of measurements of the pitch of a bowed fork; I bring it before you as indicating the accuracy both of the synchronising and the time measurement.

TABLE I.

Date.	Duration of Observation.	Temperature, Centigrade.	Pitch of Fork. (Corrected to 15·5° C)
1892	min	deg	
July 16 .. .. .	2	19·67	65·1823
July 17 (1) .. .. .	4	19·17	65·1823
July 17 (2) .. .. .	4	19·65	65·1827
July 17 (3) .. .. .	5	20·05	65·1814
July 18 (1) .. .. .	4	19·56	65·1809
July 18 (2) .. .. .	4	19·92	65·1823
July 21 .. .. .	4	18·18	65·1812
July 22 .. .. .	4	17·96	65·1831
		Mean	65·1820

Extreme variation from mean is about 1 in 65,000

When I first began working at this method I found it extremely difficult when the disc was in rotation to obtain a steady reading at the galvanometer. My galvanometer is a very sensitive Thomson reflecting galvanometer of about 1 ohm resistance, made by Elliott Bros. and fitted by my assistant, Mr. Harrison, with a long suspension for the support of the needle by a quartz fibre. When the disc was in uniform rotation the spot of light roamed continually over 50 or 100 divisions of the scale, and frequently there were sudden jerks of a most embarrassing kind. Readings with a galvanometer so sensitive were in fact impossible. (The scale divisions are 40 to the inch, and the scale is about 40 inches from the galvanometer.)

Now the electromotive-force in the circuit of the galvanometer when there is no current through the standard coil is due to three causes:—

1. The cutting of the earth's flux of induction by the radius of the rotating disc. The electromotive-force due to this cause is small in my apparatus, owing to the fact that the plane of my disc nearly coincides with the magnetic meridian. For a uniform rate of rotation it is constant.

2. The thermo-electric forces due to the general distribution of temperature in the galvanometer circuit. Changes in this distribution would be gradual, and would not account for the sudden irregular movements of the galvanometer needle.

3. The thermo-electric force at the contact of the external brush with the circumference of the disc. The sudden movements of the needle were obviously due to variations in this thermo-electric force.

I made many experiments with a view of diminishing these changes. I first tried an ordinary phosphor-bronze brush made of a

number of layers of thin sheet and controlled by a spring, brush and disc circumference being well amalgamated. This was not at all successful. Trials were then made with amalgamated copper and amalgamated lead, and with the substitution of a dead-weight pressure for a spring. But no satisfactory result could be obtained.

It was noticed, however, that after amalgamation the readings were fairly steady for a short interval, and it seemed likely that if mercury could be continuously supplied to the surface of contact between the brush and disc the electromotive-force at their contact would be rendered much more constant. This led on to the idea of

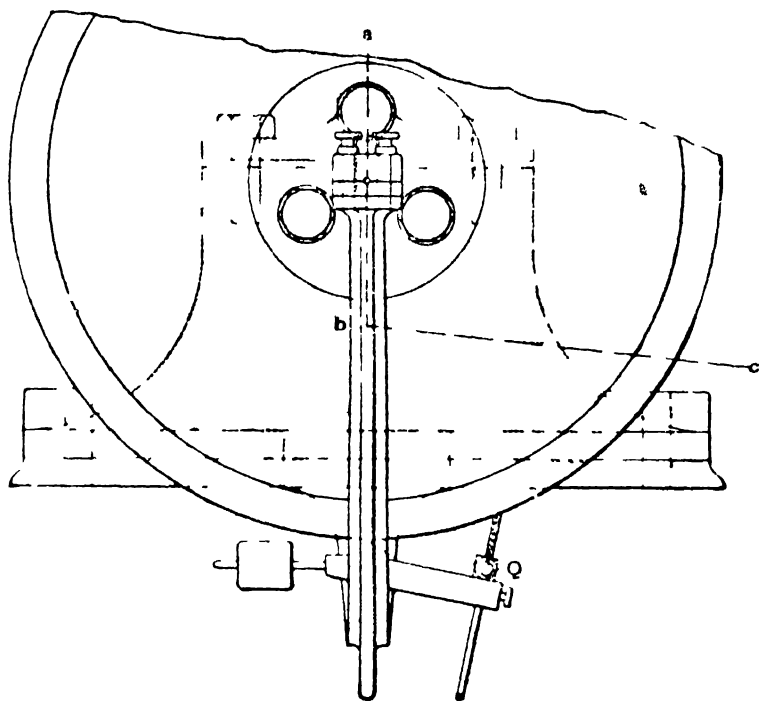


FIG. 5 —Part end View.

a brush consisting of a single wire, perforated by a channel through which a constant flow of mercury might be maintained from a cistern of adjustable height, and a brush of this description was made (Fig. 5). Trial showed that our difficulty was at an end. The variations almost disappeared, the sudden jumps quite ceased to trouble us, and the motion of the needle did not extend over more than one or two divisions of the scale for many minutes. A multiplication of such brushes round the circumference would make still further improvement.

In measuring a resistance we have to determine what I have called the equilibrium rate of rotation corresponding to that resistance, i. e. the rate of rotation such that, on reversing the direction of the current through coil and resistance by the commutator, the reading of the galvanometer is unchanged.

Now, it is not very easy to hit this exactly without making a great many trials, and, fortunately, it is not necessary for us to do so. The current produced in the galvanometer circuit by the difference of the electromotive-forces at the disc and at the resistance is proportional to that difference, and, if the current is small, it is proportional to the scale deflection on reversal. We shall commit no appreciable error, therefore, if we are near the equilibrium rate, in assuming that the difference between the equilibrium rate and the actual rate is proportional to the scale deflection on reversal, or, what comes to the same thing, that the difference  $R - Mn$  is proportional to the scale deflection on reversal.

If we take four or five rates of rotation near the equilibrium rate, and determine the deflections on reversal corresponding to each, we can by interpolation determine the rate of rotation that would correspond to no deflection, i. e. the equilibrium rate. If, say, we take five such rates, some on one side and some on the other of the equilibrium rate, we get five equations as follows:—

$$\begin{aligned} R - Mn_1 &= Cl_1, \\ R - Mn_2 &= Cl_2, \\ R - Mn_3 &= Cl_3, \\ R - Mn_4 &= Cl_4, \\ R - Mn_5 &= Cl_5, \end{aligned}$$

where  $l_1, l_2, l_3, l_4, l_5$  are the deflections corresponding to rates of rotation  $n_1, n_2, n_3, n_4, n_5$ , and  $C$  is a constant.

Combining these by the method of least squares, we may readily obtain the most probable values of  $R$  and  $C$ .

If the greatest accuracy is not required, we may content ourselves with two observations, one on one side of the equilibrium rate, and the other on the other side.

The elementary observation in the determination of a resistance with any given apparatus is, then, the determination of scale deflection on current reversal corresponding to a rate of rotation simultaneously measured.

To obtain this scale deflection accurately we must obtain it as a mean of the values given for it by a number of successive reversals succeeding one another as quickly as possible, in order to eliminate slight variations in the position of the galvanometer needle due to the slight remaining variations in the thermo-electric force at the external brush contact, and the slight variations of speed that the observer at the tuning-fork has to correct. It is therefore best not to wait for the needle to come even approximately to rest after the



disturbance due to the induction current on reversal; but to take the readings for the extreme positions in an oscillation, and having previously found the coefficient of damping, to calculate the position of rest from these two readings.

Here is a table of an average set of such observations taken during a three minutes' run. The readings with the commutator in one direction are denoted by E, and those with the commutator in the other direction are denoted by W. The deflections on reversal are given in the column marked W - E.

TABLE II.

E.		W.		E. (Calc.)	W. (Calc.)	W - E.
- 33 0	+ 19.5	+ 14.5	- 19.0	- 2.3	- 5.1	- 2.8
- 28.0	+ 17.0	+ 16.5	- 18.5	- 1.7	- 4.0	- 3.4
- 28.5	+ 15.0	+ 15.0	- 21.5	- 3.0	- 6.4	- 2.3
- 28.5	+ 11.0	+ 17.0	+ 22.0	- 3.6	- 5.8	- 1.0
- 27.0	+ 14.0	+ 17.0	- 22.5	- 3.0	- 6.1	- 3.4
- 29.0	+ 15.0	+ 19.0	- 23.5	- 3.3	- 5.9	- 2.8
- 27.0	+ 15.0	+ 18.5	- 22.5	- 2.4	- 5.5	- 2.8
- 26.0	+ 13.0	+ 18.0	- 22.0	- 3.2	- 5.4	- 3.1
- 28.5	+ 15.0	+ 18.5	- 22.5	- 3.0	- 5.5	- 2.8
- 29.5	+ 15.0	+ 16.5	- 23.0	- 3.5	- 6.6	- 2.3
- 26.5	+ 13.5	+ 22.5	- 24.5	- 3.1	- 5.0	- 2.0
- 26.5	+ 14.5	+ 19.0	- 22.5	- 2.5	- 5.3	- 3.1
- 29.5	+ 15.0	+ 16.5	- 21.0	- 3.5	- 5.4	- 3.5
- 26.5	+ 14.5	+ 18.5	- 21.5	- 2.5	- 4.9	- 1.9
- 28.5	+ 15.5	+ 17.0	+ 22.0	- 2.8	- 5.8	- 2.5
- 22.5	+ 10.5	+ 22.5	- 25.0	- 3.2	- 5.3	- 2.8
- 26.0	+ 13.5	+ 18.5	- 22.5	- 2.9	- 5.5	- 1.8
Coeff. of Damping = $\frac{1}{1.41}$ .						- 2.6
Mean .. ..						- 2.57

Summing up, then, we have during an observation one observer controlling the speed by the tuning-fork method I have described; another at the galvanometer continually reversing the commutator and calling out the galvanometer readings; at the same time the rate of rotation is being recorded on the tape of the Bain's telegraph instrument under the watchful care of a third; and a fourth person is required to write down the readings called out from the galvanometer. The run usually lasts one, two, or three minutes according to the degree of accuracy required.

The concordance of the results obtained from successive runs in measuring the same resistance is the best test of the success of the combination.

The following is a set of observations of a resistance of about  $200^1_{00}$  of an ohm made in July and August, 1893:—

TABLE III.

July 17th, morning .. .. .	0·00050016
July 17th, afternoon .. .. .	0·00050016
July 19th, morning .. .. .	0·00050015
August 2nd, afternoon .. .. .	0·00050020
August 3rd, morning .. .. .	0·00050021
August 4th, morning .. .. .	0·00050016
August 4th, afternoon .. .. .	0·00050013
August 5th, morning .. .. .	0·00050019
August 9th, morning .. .. .	0·00050021
August 9th, afternoon .. .. .	0·00050018
Mean .. .. .	0·00050017

The maximum divergence from the mean is 0·00000004, or about one part in 12,000.

Here is another set, made last year in determining the value of the international ohm in absolute units.

The results are as follows, the figure in each case giving the value of the international ohm in true ohms:—

July 7th.—Standard coil carefully adjusted. Three-minute tapes.

	0·999703
	0·999761
	0·999807
Mean .. .. .	0·999757

July 9th.—No readjustment of standard coil. One-minute tapes.

	0·999757
	0·999711
	0·999683
	0·999782
Mean .. .. .	0·999733

July 10th, morning.—Standard coil readjusted. One-minute tapes.

0·999734  
0·999818  
0·999726

Mean .. .. 0·999759

July 10th, afternoon.—No readjustment of standard coil. Three-minute tapes.

0·999708  
0·999742  
0·999764

Mean .. .. 0·999738

July 11th, afternoon.—Standard coil readjusted. Three-minute tapes.

0·999693  
0·999692  
0·999679

Mean .. .. 0·999688

July 12, morning.—No readjustment of standard coil. Resistance coils reversed.

0·999713  
0·999711  
0·999692

Mean .. .. 0·999705

July 12th, afternoon.—Standard coil readjusted. Resistance coils removed from the mercury cups and replaced. Three minute tapes.

0·999774  
0·999787  
0·999759

Mean .. .. 0·999773

July 13th.—Standard coil readjusted. Resistance coils removed from mercury cups and replaced. Three-minute tapes.

0·999847  
0·999809  
0·999782  
0·999842 (morning of the 14th)

Mean .. .. 0·999820

July 14th, morning.—Standard coil readjusted. Resistance coils removed and replaced. Three-minute tapes.

0·999695  
0·999692  
0·999717

Mean .. .. 0·999701

July 14th, afternoon.—Standard coil readjusted. Resistance coils removed and replaced. Three-minute tapes.

		0·999853
		0·999866
		0·999875
Mean	.. ..	0·999865

These results show that no single observation differs from the mean of a number by more than one part in ten thousand.

We have now to consider the other factor in the resistance—the coefficient of mutual induction of the coil and disc. This depends merely on the dimensions of the coil and disc if they are accurately centred, and as the adjustment for centre was made in between the successive sets given in the last table, we may take for granted that it can be made with the requisite accuracy.

The circumference of the disc is a sufficiently true circle, the disc having been ground true in place. Disc and axle were then removed and transferred to my Whitworth measuring machine. The measurement of the disc's diameter presented no difficulty. It was easily determined to the ten-thousandth of an inch. The mean radius of my coil cannot be determined with the same accuracy, but I believe it is known to the thousandth of an inch. The coil consists of a single layer of silk-covered wire wound in a screw thread cut in a brass frame. The silk covering of the wire introduces some uncertainty. It was measured bolted to its stand as in use. Measurements were taken along 18 diameters in the Whitworth machine with the following results:—

Diameter.	Measurement	Diameter	Measurement.
deg.		deg.	
0-180	21·0898	90-270	21·1038
10-190	21·0929	100-280	21·1056
20-200	21·0951	110-290	21·1041
30-210	21·0933	120-300	21·1014
40-220	21·0960	130-310	21·0979
50-230	21·0998	140-320	21·0945
60-240	21·1017	150-330	21·0924
70-250	21·1026	160-340	21·0900
80-260	21·1044	170-350	21·0910
Max., 21·1056		Mean, 21·09757	
Min, 21·0898		$t = 17^{\circ} \text{C.}$	
0 0158			

These measures clearly show that the coil is elliptical in section, the difference between the major and minor axes being about 0·008 in., or about one part in 1300. The way in which this fact emerges from

the measurements, is, however, some indication of the accuracy with which the individual measurements in the machine can be made, even in the case of a coil of insulated wire.

My coil, then, is faulty on account of this ellipticity. I have not yet calculated the possible error due to this cause; it cannot be very great. How much it is only calculation can settle; and the calculation is not without difficulty. But this, after all, is only a question of my apparatus, and does not affect the general question of the possible accuracy of the method.

A more solid metal frame might, no doubt, be turned true to the accuracy required, and if so turned might then be measured with the requisite accuracy in the Whitworth machine.

A still further improvement may be made by making the coil frame of insulating material, say paraffined marble, and winding on it naked wire instead of silk-covered wire. The radius might then be measured with certainty to something much better than 1 part in 10,000. The use of a coil with a single layer of wire instead of many layers greatly facilitates the determination of the mean radius. It, of course, necessitated the finding of a new formula for the coefficient of mutual induction.

This I obtained by direct integration of the general integral for the case in point, viz. a circle and coaxial helix. The coefficient of mutual induction is given us as a sufficiently converging series involving elliptical or quasi-elliptical integrals.

We may, then, in the result conclude that, though I cannot guarantee my own coefficient of mutual induction as correct to 1 part in 10,000 till I have calculated the effect of the coil ellipticity, it lies well within the resources of our mechanical engineers to make a coil and disc free from uncertainty to this degree of accuracy. And if this be so I am warranted in stating that a resistance can be measured in absolute units to 1 part in 10,000.

Now, in the Order in Council, from which I quoted in the early part of my discourse, it is stated that in the use of the ohm standard the limit of accuracy attainable is one-hundredth part of 1 per cent.—i. e. one part in 10,000. Hence the Government gains nothing in precision to compensate for the risk it has taken in adopting as unit the resistance of a standard coil, which may vary from time to time in consequence of changes in the physical condition of the coil.

There is no valid ground left for adopting as ultimate unit any other unit than the absolute unit itself. We have not in our electrical standard legislation given full credit to the mechanical engineer for what he can do for us. He can make a machine that will measure resistance in absolute units with a precision as great as—1 might even say greater than—that with which the Government is prepared to guarantee its comparisons. Such a machine ought to be at work in the Board of Trade laboratory, in order that there may be opportunity from time to time, at regular intervals, to measure the Government coil or coils in absolute units. It is necessary that this should be

done if we are to be guarded against the perpetual inconvenience of unknown possible changes in the coil, the resistance of which is now the legal unit.

Such a step would have collateral advantages. It would enable the Board of Trade to certify standards of low electrical resistance. With such a machine standards of from a thousandth to a two-hundred-thousandth of an ohm may be measured to nearly the same percentage accuracy. All we have to do in dealing with the very low resistances is to pass a sufficiently large current through them, and shunt the standard coil of the machine. But to enter into details on this point would lead me too far. I must content myself with saying that I believe such a machine is much the best instrument for standardising low electrical resistances, and that accurate standards of low resistance would be of great service both in the laboratory and the workshop.

I have, in conclusion, only to express my obligation to my assistant, Mr. Samuel Harrison, for the great and constant help he has given me in the course of my investigations on this subject.

[J. V. J.]

Friday, January 17, 1896.

SIR FREDERICK ABEL, Bart. K.C.B. D.C.L. LL.D. F.R.S.  
Vice-President, in the Chair.

THE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S.  
*M.R.I.* Professor of Natural Philosophy R.I.

*More about Argon.*

(Abstract.)

IN our original paper\* are described determinations by Professor Ramsay, of the density of argon prepared with the aid of magnesium. The volume actually weighed was 163 c.c. and the adopted mean result was 19.941, referred to  $O_2 = 16$ . At that time a satisfactory conclusion as to the density of argon prepared by the oxygen method of Cavendish had not been reached, although a preliminary result (19.7) obtained from a mixture of argon and oxygen† went far to show that the densities of the gases prepared by the two methods were the same. In order further to test the identity of the gases, it was thought desirable to pursue the question of density; and I determined, as the event proved, somewhat rashly, to attempt large scale weighings of pure argon with the globe of 1800 c.c. capacity employed in former weighings of gases‡ which could be obtained in quantity.

The accumulation of the 3 litres of argon, required for convenient working, involved the absorption of some 300 litres of nitrogen, or about 800 litres of the mixture with oxygen. This was effected at the Royal Institution with the apparatus already described,§ and which is capable of absorbing the mixture at the rate of about 7 litres per hour. The operations extended themselves over nearly three weeks, after which the residual gases amounting to about 10 litres, still containing oxygen with a considerable quantity of nitrogen, were removed to the country and transferred to a special apparatus where it could be prepared for weighing.

For this purpose the purifying vessel had to be arranged somewhat differently from that employed in the preliminary absorption

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\* Rayleigh and Ramsay, Phil. Trans. vol. 186 A, pp. 221, 238, 1895.

† Loc. cit. p. 221.

‡ Roy. Soc. Proc. February 1888; February 1892; March 1893.

§ Phil. Trans. loc. cit. p. 219.

of nitrogen. When the gas is withdrawn for weighing, the space left vacant must be filled up with liquid, and afterwards when the gas is brought back for repurification, the liquid must be removed. In order to effect this the working vessel (Fig. 7\*) communicates by means of a siphon with a 10-litre "aspirating bottle," the ends of the siphon being situated in both cases near the bottom of the liquid. In this way the alkaline solution may be made to pass backwards and forwards, in correspondence with the desired displacements of gas.

There is, however, one objection to this arrangement which requires to be met. If the reserve alkali in the aspirating bottle were allowed to come into contact with air, it would inevitably dissolve nitrogen, and this nitrogen would be partially liberated again in the working vessel, and so render impossible a complete elimination of that gas from the mixture of argon and oxygen. By means of two more aspirating bottles an atmosphere of *oxygen* was maintained in the first bottle, and the outermost bottle, connected with the second by a rubber hose, gave the necessary control over the pressure.

Five glass tubes in all were carried through the large rubber cork by which the neck of the working vessel was closed. Two of these convey the electrodes: one is the siphon for the supply of alkali, while the fourth and fifth are for the withdrawal and introduction of the gas, the former being bent up internally, so as to allow almost the whole of the gaseous contents to be removed. The fifth tube, by which the gas is returned, communicates with the fall-tube of the Topley pump, provision being made for the overflow of mercury. In this way the gas, after weighing, could be returned to the working vessel at the same time that the globe was exhausted. It would be tedious to describe in detail the minor arrangements. Advantage was frequently taken of the fact that *oxygen* could always be added with impunity, its presence in the working vessel being a necessity in any case.

When the nitrogen had been so far removed that it was thought desirable to execute a weighing, the gas on its way to the globe had to be freed from oxygen and moisture. The purifying tubes contained copper and copper oxide maintained at a red heat, caustic soda, and phosphoric anhydride. In all other respects the arrangements were as described in the memoir on the densities of the principal gases,† the weighing globe being filled at 0°, and at the pressure of the manometer gauge.

The process of purification with the means at my command proved to be extremely slow. The gas contained more nitrogen than had been expected, and the contraction went on from day to day until I almost despaired of reaching a conclusion. But at last the visible contraction ceased, and soon afterwards the yellow line of nitrogen

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\* Phil. Trans. loc. cit. p. 218.

† Roy. Soc. Proc. vol. 53, p. 134, 1893.



disappeared from the spectrum of the jar discharge.\* After a little more sparking, a satisfactory weighing was obtained on May 22, 1895; but, in attempting to repeat, a breakage occurred, by which a litre of air entered, and the whole process of purification had to be recommenced. The object in view was to effect, if possible, a *series* of weighings with intermediate sparkings, so as to obtain evidence that the purification had really reached a limit. The second attempt was scarcely more successful, another accident occurring when two weighings only had been completed. Ultimately a series of four weighings were successfully executed, from which a satisfactory conclusion can be arrived at.

May 22	..	..	..	..	..	..	3.2710
June 4	..	..	..	..	..	..	3.2617
June 7	..	..	..	..	..	..	3.2727
June 13	..	..	..	..	..	..	3.2652
June 18	..	..	..	..	..	..	3.2750
June 25	..	..	..	..	..	..	3.2748
July 2	..	..	..	..	..	..	3.2741

} 3.2746

The results here recorded are derived from the comparison of the weighings of the globe "full" with the mean of the preceding and following weighings "empty," and they are corrected for the errors of the weights and for the shrinkage of the globe when exhausted, as explained in former papers. In the last series, the experiment of June 13 gave a result already known to be too low. The gas was accordingly sparked for fourteen hours more. Between the weighings of June 18 and June 25 there was nine hours' sparking, and between those of June 25 and July 2 about eight hours' sparking. The mean of the last three, viz. 3.2746, is taken as the definitive result, and it is immediately comparable with 2.6276, the weight under similar circumstances of oxygen.† If we take  $O_2 = 16$ , we obtain for argon

$$19.940,$$

in very close agreement with Professor Ramsay's result

The conclusion from the spectroscopic evidence that the gases isolated from the atmosphere by magnesium and by oxygen are essentially the same is thus confirmed.

The refractivity of argon was next investigated, in the hope that it might throw some light upon the character of the gas. For this

\* Jan. 29.—When the argon is nearly pure, the arc discharge (no jar connected) assumes a peculiar purplish colour, quite distinct from the greenish hue apparent while the oxidation of nitrogen is in progress and from the sky blue observed when the residue consists mainly of oxygen.

† Roy. Soc. Proc. vol. 53, p. 144, 1893.

purpose absolute measurements were not required. It sufficed to compare the pressures necessary in two columns of air and argon of equal lengths, in order to balance the retardations undergone by light in traversing them.

The arrangement was a modification of one investigated by Fraunhofer, depending upon the interference of light transmitted through two parallel vertical slits placed in front of the object-glass of a telescope. If there be only one slit, and if the original source, either a distant point or a vertical line of light, be in focus, the field is of a certain width, due to "diffraction," and inversely as the width of the slit. If there be two equal parallel slits whose distance apart is a considerable multiple of the width of either, the field is traversed by bands of width inversely as the distance between the slits. If from any cause one of the portions of light be retarded relatively to the other, the bands are displaced in the usual manner, and can be brought back to the original position only by abolishing the relative retardation.

When the object is merely to see the interference bands in full perfection, the use of a telescope is not required. The function of the telescope is really to magnify the slit system,\* and this is necessary when, as here, it is desired to operate separately upon the two portions of light. The apparatus is, however, extremely simple, the principal objection to it being the high magnifying power required, leading under ordinary arrangements to a great attenuation of light. I have found that this objection may be almost entirely overcome by the substitution of cylindrical lenses, magnifying in the horizontal direction only, for the spherical lenses of ordinary eye-pieces. For many purposes a single lens suffices, but it must be of high power. In the measurements about to be described most of the magnifying was done by a lens of home manufacture. It consisted simply of a round rod, about  $\frac{1}{8}$  inch (4 mm.) in diameter, cut by Mr. Gordon from a piece of plate glass.† This could be used alone; but as at first it was thought necessary to have a web, serving as a fixed mark to which the bands could be referred, the rod was treated as the object-glass of a compound cylindrical microscope, the eye-piece being a commercial cylindrical lens of  $1\frac{1}{4}$  inch (31 mm.) focus. Both lenses were mounted on adjustable stands, so that the cylindrical axes could be made accurately vertical, or, rather, accurately parallel to the length of the original slit. The light from an ordinary paraffin lamp now sufficed, although the magnification was such as to allow the error of setting to be less than  $1/20$  of a band interval. It is to be remembered that with this arrangement the various parts of the length of a band correspond, not to the various parts of the original slit, but rather to the various parts of the object-glass. This

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\* Brit. Assoc. Report, 1893, p. 703.

† Preliminary experiments had been made with ordinary glass cane and with tubes charged with water.

departure from the operation of a spherical eye-piece is an advantage, inasmuch as optical defects show themselves by deformation of the bands instead of by a more injurious encroachment upon the distinction between the dark and bright parts.

The collimating lens A (Fig. 1) is situated 23 feet (7 metres) from the source of light. B, C are the tubes, one containing dry air, the other the gas to be experimented upon. They are 1 foot (30.5 cm.) long, and of  $\frac{1}{2}$  inch (1.3 cm.) bore, and they are closed at the ends with small plates of parallel glass cut from the same strip. E is the object-glass of the telescope, about 3 inches (7.6 cm.) in diameter. It is fitted with a cap D, perforated by two parallel slits. Each slit is  $\frac{1}{4}$  inch (6 mm.) wide, and the distance between the middle lines of the slits is  $1\frac{1}{2}$  inches (38 mm.).

The arrangements for charging the tubes and varying the pressures of the gases are sketched in Fig. 2. A gas pipette, D E, communicates with the tube C, so that by motion of the reservoir E and consequent flow of mercury through the connecting hose, part of the gas may be transferred. The pressure was measured by a U-shaped

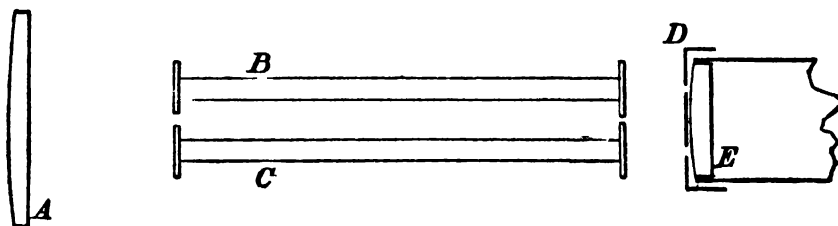


FIG. 1.

manometer F, containing mercury. This was fitted below with a short length of stout rubber tubing G, to which was applied a squeezer H. The object of this attachment was to cause a rise of mercury in both limbs immediately before a reading, and thus to avoid the capillary errors that would otherwise have entered. A similar pipette and manometer were connected with the air tube B. In order to be able, if desired, to follow with the eye a particular band during the changes of pressure (effected by small steps and alternately in the two tubes), diminutive windlasses were provided by which the motions of the reservoirs (E) could be made smooth and slow. In this way all doubt was obviated as to the identity of a band; but after a little experience the precaution was found to be unnecessary.

The manner of experimenting will now be evident. By adjustment of pressures the centre of the middle band was brought to a definite position, determined by the web or otherwise, and the pressures were measured. Both pressures were then altered and adjusted until the band was brought back precisely to its original position. The ratio of the changes of pressure in the inverse ratio of the refractivities

( $\mu = 1$ ) of the gases. The process may be repeated backwards and forwards any number of times, so as to eliminate in great degree errors of the settings and of the pressure readings.

During these observations a curious effect was noticed, made possible by the independent action of the parts of the object-glass situated at various levels, as already referred to. When the bands were stationary, they appeared straight, or nearly so, but when in motion, owing to changes of pressure, they became curved, even in passing the fiducial position, and always in such a manner that the

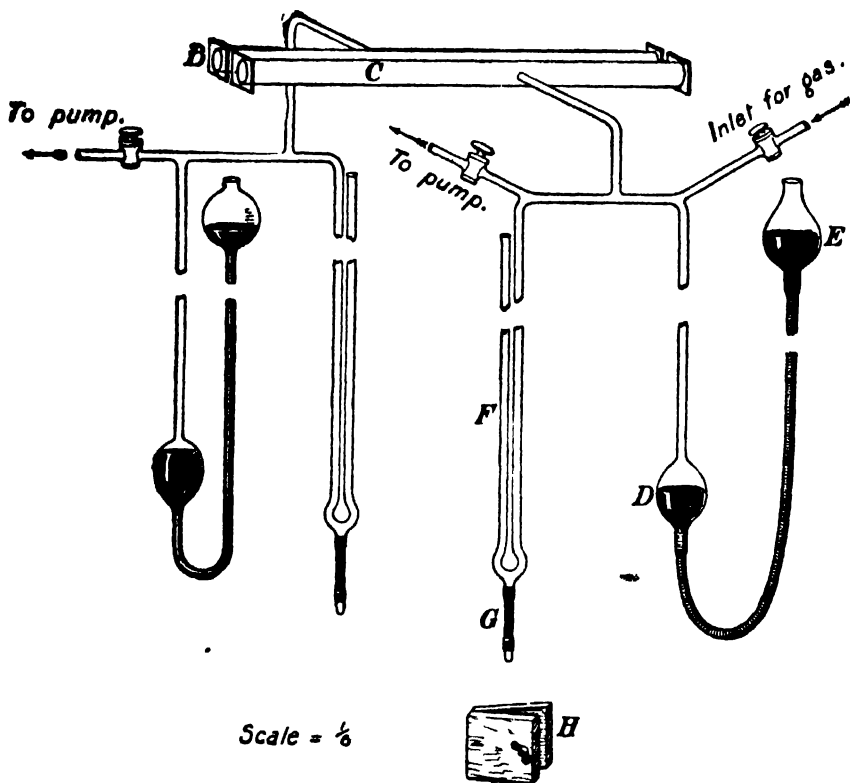


FIG. 2.

ends led. The explanation is readily seen to depend upon the temporary changes of temperature which accompany compression or rarefaction. The full effect of a compression, for example, would not be attained until the gas had cooled back to its normal temperature, and this recovery of temperature would occur more quickly at the top and bottom, where the gas is in proximity to the metal, than in the central part of the tube.

The success of the measures evidently requires that there should be no apparent movement of the bands apart from real retardations

in the tubes. As the apparatus was at first arranged, this condition was insufficiently satisfied. Although all the parts were carried upon the walls of the room, frequent and somewhat sudden displacements of the bands relatively to the web were seen to occur, probably in consequence of the use of wood in some of the supports. The observations could easily be arranged in such a manner that no systematic error could thence enter, but the agreement of individual measures was impaired. Subsequently a remedy was found in the use of a second system of bands, formed by light which passed just above the tubes, to which, instead of to the web, the movable bands were referred. The coincidence of the two systems could be observed with accuracy, and was found to be maintained in spite of movements of both relatively to the web.

In the comparisons of argon and air (with nearly the same refractivities) the changes of pressure employed were about 8 inches (20 cm.), being deductions from the atmospheric pressure. In one observation of July 26, the numbers, representing suction in inches of mercury, stood

Argon.	Air.
8.54	99.6
0.01	1.77
<hr/>	<hr/>
8.53	8.19

$$\text{Ratio} = 0.961,$$

signifying that 8.53 inches of argon balanced 8.19 inches of dry air. Four sets, during which the air and argon (from the globe as last filled for weighing) were changed, taken on July 17, 18, 19, 26, gave respectively for the final ratio 0.962, 0.961, 0.961, 0.960, or as the mean

$$\frac{\text{Refractivity of argon}}{\text{Refractivity of air}} = 0.961.$$

The evidence from the refractivities, as well as from the weights, is very unfavourable to the view that argon is an allotropic form of nitrogen such as would be denoted by  $N_3$ .

The above measurements, having been made with lamp light, refer to the most luminous region of the spectrum, say in the neighbourhood of D. But since no change in the appearance of the bands at the two settings could be detected, the inference is that the displacements of the two gases are approximately the same, so that the above ratio would not be much changed, even if another part of the spectrum were chosen. It may be remarked that the displacement actually compensated in the above experiments amounted to about forty bands, each band corresponding to about  $\frac{1}{2}$  inch (5 mm.) pressure of mercury.

Similar comparisons have been made between air and helium.

The latter gas, prepared by Professor Ramsay, was brought from London by Mr. W. Randall, who further gave valuable assistance in the manipulations. It appeared at once that the refractivity of helium was remarkably low, 13 inches pressure of the gas being balanced by less than 2 inches pressure of air. The ratios given by single comparisons on July 29 were 0·147, 0·146, 0·145, 0·146, mean 0·146; and on July 30, 0·147, 0·147, 0·145, 0·145, mean 0·146. The observations were not made under ideal conditions, on account of the smallness of the changes of air pressure; but we may conclude that with considerable approximation

$$\frac{\text{Refractivity of helium}}{\text{Refractivity of air}} = 0\cdot146.$$

The lowest refractivity previously known is that of hydrogen, nearly 0·5 of that of air.

The viscosity was investigated by the method of passage through capillary tubes. The approximate formula has been investigated by O. Meyer,\* on the basis of Stokes' theory for incompressible fluids. If the driving pressure ( $p_1 - p_2$ ) is not too great, the volume  $V_2$  delivered in time  $t$  through a tube of radius  $R$  and length  $\lambda$  is given by

$$V_2 = \pi t \frac{p_1^2 - p_2^2}{2p_2} \frac{R^4}{8\eta\lambda},$$

the volume being measured at the lower pressure  $p_2$ , and  $\eta$  denoting the viscosity of the gas. In the comparison of different gases  $V_2$ ,  $p_1$ ,  $p_2$ ,  $R$ ,  $\lambda$  may be the same, and then  $\eta$  is proportional to  $t$ .

In the apparatus employed two gas pipettes and manometers, somewhat similar to those shown in Fig. 2, were connected by a capillary tube of very small bore and about 1 metre long. The volume  $V_2$  was about 100 c.c. and was caused to pass by a pressure of a few centimetres of mercury, maintained as uniform as possible by means of the pipettes. There was a difficulty, almost inherent in the use of mercury, in securing the right pressures during the first few seconds of an experiment; but this was not of much importance as the whole time  $t$  amounted to several minutes. The apparatus was tested upon hydrogen, and was found to give the received numbers with sufficient accuracy. The results, referred to dry air, were for helium 0·96; and for argon 1·21, somewhat higher than for oxygen which at present stands at the head of the list of the principal gases.

In the original memoir upon argon† results were given of weighings of the residue from the Bath gas after removal of oxygen, carbonic anhydride, and moisture, from which it appeared that the

\* Pogg Ann. vol. 127, p 270, 1866

† Rayleigh and Ramsay, Phil. Trans. A, vol. 186, p 227, 1895.

proportion of argon was only one-half of that contained in the residue, after similar treatment from the atmosphere. After the discovery of helium by Professor Ramsay, the question presented itself as to whether this conclusion might not be disturbed by the presence in the Bath gas of helium, whose lightness would tend to compensate the extra density of argon.

An examination of the gas which had stood in my laboratory more than a year having shown that it still contained no oxygen, it was thought worth while to remove the nitrogen so as to determine the proportion that would refuse oxidation. For this purpose 200 c.c. were worked up with the oxygen until the volume, free from nitrogen, was reduced to 8 c.c. On treatment with pyrogallol and alkali the residue measured 3.3 c.c. representing argon, and helium, if present. On sparking the residue at atmospheric pressure and examining the spectrum, it was seen to be mainly that of argon, but with an unmistakable exhibition of  $D_3$ . At atmospheric pressure this line appears very diffuse in a spectroscope of rather high power, but the place was correct.

From another sample of residue from the Bath gas, vacuum tubes were charged by my son, Mr. R. J. Strutt, and some of them showed  $D_3$  sharply defined and precisely coincident with the line of helium in a vacuum tube prepared by Professor Ramsay.

Although the presence of helium in the Bath gas is not doubtful, the quantity seems insufficient to explain the low density found in October 1894. In order to reconcile that density with the proportion of residue ( $3.3/200 = 0.016$ ) found in the experiment just described, it would be necessary to suppose that the helium amounted to 25 per cent. of the whole residue of argon and helium. Experiment, however, proved that a mixture of argon and helium containing 10 per cent. of the latter gas showed  $D_3$  more plainly than did the Bath residue. It is just possible that some of the helium was lost by diffusion during the long interval between the experiments whose results are combined in the above estimate.

Gas from the Buxton springs, kindly collected for me by Mr. A. McDougall, was found to contain no appreciable oxygen. The argon amounted to about 2 per cent. of the volume. When its spectrum was examined, the presence of  $D_3$  was suspected, but the appearance was too feeble to allow of a definite statement being made. The proportion of helium is in any case very much lower than in the Bath gas.

Is helium contained in the atmosphere? Apart from its independent interest, this question is important in connection with the density of atmospheric argon. Since the spectrum of this gas does not show the line  $D_3$ , we may probably conclude that the proportion of helium is less than 3 per cent.; so that there would be less than  $3 \times 10^{-4}$  of helium in the atmosphere. The experiment about to be described was an attempt to carry the matter further, and is founded upon the observation by Professor Ramsay, that the solu-

bility of helium in water is only 0·007, less than one-fifth of that which we found for argon.\*

It is evident that if a mixture of helium and argon be dissolved in water until there is only a small fraction remaining over, the proportion of helium will be much increased in the residue. Two experiments have been made, of which that on October 6, 1895, was the more elaborate. About 60 c.c. of argon were shaken for a long time with well boiled water contained in a large flask. When the absorption had ceased, the residue of 30 c.c. was sparked with a little oxygen until no nitrogen could be seen in the spectrum. It was then treated a second time with boiled water until its volume was reduced to  $1\frac{1}{2}$  c.c. With this vacuum tubes were charged by my son at two different pressures. In none of them could  $D_3$  be detected; nor was there any marked difference to be seen between the spectra of the washed and the unwashed argon. If helium be present in the atmosphere, it must be in very small quantity, probably much less than a ten-thousandth part.

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\* Phil. Trans A, vol. 186, p. 225, 1895.



Friday, March 27, 1896.

EDWARD FRANKLAND, Esq. D.C.L. LL.D. F.R.S. Vice-President,  
in the Chair.

PROFESSOR DEWAR, M.A. LL.D. F.R.S. *M.R.I.*

*New Researches on Liquid Air.*

OF all the forms of engineering plant used in low temperature research, the best and most economical for the production of liquid air or oxygen is one based on the general plan of the apparatus used by Pictet in his celebrated experiments on the liquefaction of oxygen in the year 1878. Instead of using Pictet's combined circuits of liquid sulphur dioxide and carbon dioxide, maintained in continuous circulation by means of compression, liquefaction and subsequent evaporation, it is preferable to select ethylene (after Cailletet and Wroblewski) for one circuit, and for the other either nitrous oxide or, better, carbon dioxide. Further, instead of making highly compressed oxygen to be liquefied by heating potassium chlorate in an iron bomb directly connected with the refrigerator, it is safer and more convenient to use gas previously compressed in steel cylinders. The stopcock that Pictet employed to draw off liquid and produce sudden expansion, was in his apparatus placed outside the refrigerator proper, but it is now placed inside, so as to be kept cool by the gases undergoing expansion. This improvement was introduced along with that of isolating the liquid gases by surrounding them with their own cooled vapour in the apparatus made wholly of copper, described and figured in the *Proc. Roy. Inst.* for 1886. In all continuously working circuits of liquid gases used in refrigerating apparatus, the regenerative principle applied to cold, first introduced by Siemens in 1857, and subsequently employed in the freezing machines of Kirk, Coleman, Solvay, Linde and others, has been adopted. Quite independently, Professor Kamerlingh Onnes, of Leiden, has used the regenerative principle in the construction of the cooling circuits in his cryogenic laboratory.\* Apart, therefore, from important mechanical details, and the conduct of the general working, nothing new has been added by any investigator to the principles involved in the construction and use of low temperature apparatus since the year 1878.

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\* See paper by Dr. H. Kamerlingh Onnes, on the "Cryogenic Laboratory at Leiden, and on the Production of very low Temperatures," Amsterdam Akademie, 1894.

Detailed drawings of the Royal Institution refrigerating plant now in use have not been published, simply because changes are constantly being made in the apparatus. Science derives no benefit from the description of transitional apparatus when there is no secret about the working process and how to carry it into effect. The *Phil. Mag.* of February, 1895, contains a fantastic claim put forward by Professor Olszewski, of Cracow, that because he used in 1890 a steel tube combined with a stopcock to draw off liquid oxygen, he had taught the world, to use his own language, "the method of getting large quantities of liquid gases." In addition the Professor alleges, four years after the event, that the experiments made at the Royal Institution are chiefly borrowed from Cracow, and that he is entitled to the credit of all low temperature research. As to such claims, one can only wonder at the meagre additions to knowledge that in our time are unhesitatingly brought forward as original, and more especially that scientific men could be got to give them any currency in this country. Such persons should read the late Professor Wroblewski's pamphlet, entitled '*Comment l'air a été liqueté*,'\* and make themselves generally acquainted with the work of this most remarkable man before coming to hasty conclusions on claims of priority brought forward by his some time colleague.

*Liquefying Apparatus*—A laboratory apparatus for the production of liquid oxygen and other gases is represented in section (Fig. 1). With this simple machine, 100 c.c. of liquid oxygen can readily be obtained, the cooling agent being carbon dioxide, at the temperature of  $-79^{\circ}$ . If liquid air has to be made by this apparatus, then the carbonic acid must be kept under exhaustion of about 1 inch of mercury pressure, so as to begin with a temperature of  $-115^{\circ}$ . Under such conditions the yield of the liquid gases is much greater. The gaseous oxygen, cooled before expansion by passing through a spiral of copper tube immersed in solid carbon dioxide, passes through a fine screw stopcock under a pressure of 100 atmos., and thence backwards over the coils of pipe. The liquid oxygen begins to drop in about a quarter of an hour from starting. The general arrangement of the circuits will be easily understood from the sectional drawing. The pressure in the oxygen cylinders at starting is generally about 150 atmos., and the best results are got by working down to about 100. If a small compressor is combined with the apparatus the liquefaction can go on continuously. This little apparatus will enable liquid oxygen or air to be used for demonstration and research in all laboratories.

*Vacuum Vessels*.—It has been shown in previous papers† that a good exhaustion reduces the influx of heat to one-fifth part of what is conveyed when the annular space in such double-walled vacuum vessels is filled with air. If the interior walls are silvered, or excess

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\* Paris, Librairie du Luxembourg, 1885

† "On Liquid Atmospheric Air," *Proc. Roy. Inst.* 1893, "Scientific Uses of Liquid Air," *ibid.* 1894.

of mercury is left in the vessel, the influx of heat is diminished to one-sixth part of the amount entering without the metallic coating. The total effect of the high vacuum and silvering is to reduce the ingoing heat to one-thirtieth part, or, roughly,  $3\frac{1}{2}$  per cent.. Vessels constructed with three dry air spaces only reduced the influx of heat to 35 per cent. An ordinary mercury vacuum vessel is therefore ten times more economical for storing liquid air, apart from considerations of manipulation, than a triple annular spaced air vessel. It has been suggested that the metallic coating of mercury does no good, because Pictet has found that all kinds of matter become transparent to heat at low temperatures. The results above mentioned dispose of this assumption, and direct experiment proves that no increase in the transparency of glass to thermal radiation is effected by cooling it to the boiling point of air.\*

An ocular demonstration of the correctness of the above statements can easily be shown by mounting on the same stem three similar double-walled test tubes, two of which have been simultaneously exhausted and sealed off from the air pump together, while the third is left full of air. One of the vacuum test tubes is coated with silver in the interior. The apparatus is shown in Fig. 2. A has the annular space filled with air; B and C are exhausted, C being coated with silver. On filling liquid ethylene to the same height into each vessel, and inserting corks with similar gas jets and igniting the escaping gas, the relative volumes of the flames is roughly proportional to the influx of heat, and resembles what is shown in the drawing. It is satisfactory to have independent corroboration of the advantages of the use of vacuum vessels, and this may be found in a paper by Professor Kamerlingh Onnes, of Leiden, communicated to the Amsterdam Academy of Sciences, 1896, entitled 'Remarks on the Liquefaction of Hydrogen, on Thermodynamical Similarity, and in the Use of Vacuum Vessels,' in which he says:—"In the same degree as it becomes of more importance to effectuate adiabatic processes at very low temperatures, the importance of the vacuum vessels of Dewar will increase. It seems to me that they are the most important addition since 1883 to the appliances for low temperature research." . . . "It is a rejoicing prospect that practical engineers will doubtless feel the want of such non-conducting mantles. For as soon as this stage is

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\* At a meeting of the French Academy in 1895 a paper by M. Solvay of Brussels was read, in which my 1892 device of vacuum vessels was attributed to M. Cailletet, and tacitly accepted by him! In 1875 I had already used a highly exhaustive vessel, of similar shape to the vacuous test tube, in calorimetric experiments. See paper on "The Physical Constants of Hydrogenium," Trans. Roy. Soc. Ed. vol. xxvii. Even as late as April 1896, Professor Tilden, D.Sc. F.R.S. of the Royal College of Science, in a paper entitled "L'Appareil du Dr. Hampson pour la Liquefaction de l'air et des gaz," communicated to the 'Revue Générale des Sciences,' thought proper to write as follows: "Un manchon de verre, dans lequel on a fait le vide (manchon semblable à ceux décrits par Cailletet ou Dewar)." Where did Professor Tilden find Cailletet's description of a vacuum vessel? This is not the only statement in the paper requiring correction.

reached, numbers of heads and hands are disposed to take over the problem from the scientific researcher."

*Solid Air.*—As Professor Olszewski has recently alleged that air does not solidify at the lowest pressures,\* the author's former experiments were repeated on a larger scale. If a litre of liquid air is placed in a globular silvered vacuum vessel and subjected to exhaustion, as much as half a litre of solid air can be obtained and maintained in this condition for half an hour. At first the solid is a stiff, transparent jelly, which, when examined in the magnetic field, has the liquid oxygen drawn out of it to the poles. This proves that solid air is a nitrogen-jelly containing liquid oxygen. This statement was made in a paper "On the Refraction and Dispersion of Liquid Oxygen, and the Absorption Spectrum of Liquid Air" (Professors Liveing and Dewar), published in the *Phil. Mag.* for September 1895, yet Professor Olszewski, in 1896† is declaring "that Professor Dewar has stated that liquid air solidifies as such, the solid product containing a slightly smaller percentage of nitrogen than is present in the atmosphere. My experiments have proved this statement to be incorrect." The Cracow professor may well have the satisfaction of correcting a statement which was never made by me. He seems also to forget that in 1893, *Proc. Roy. Inst. Lecture on Liquid Air*, it is distinctly stated that "all attempts to solidify oxygen by its own evaporation have failed." Solid air can only be examined in a vacuum or in an atmosphere of hydrogen, because it instantly melts on exposure to air cooled to the temperature of its boiling point, giving rise to the liquefaction of an additional quantity of air. It is strange to see a mass of solid air melting in contact with the atmosphere, and all the time welling up like a kind of fountain. The apparatus shown in Fig. 3 is well adapted for showing the direct liquefaction of the air of a room and its solidification. A large vacuum vessel G, is mounted on a brass stand containing another smaller vessel B of the same kind. By means of the two cocks C and D, either the large vessel G or the bulb B can be connected to the air pump circuit. Liquid oxygen is placed in A, which can, by opening the stopcock D, be cooled to  $-210^{\circ}$  by exhaustion. If the stopcock C is shut and a barometric gage is joined on at F, the dropping of the liquid air from the outside of A will go on even at as low a pressure as 4 in. of mercury; which is equivalent to saying that this apparatus would liquefy air if taken by a balloon ten miles high. If F is now opened, giving a supply of air at atmospheric pressure, the cup B soon fills with liquid air. Unless the air supply is passed over soda lime and strong sulphuric, the liquid is always turbid from the presence of ice crystals and solid carbonic acid. Now on shutting F and opening C, the air in B is placed under exhaustion and soon solidifies to a jelly-like mass. When the vacuum is about 14 mm. then the temperature of the solid air is  $-232^{\circ}$  by the platinum resistance

\* *Phil. Mag.* February 1895.

† See 'Nature,' Aug. 20, p. 378.

thermometer, or  $-216^{\circ}\text{C}$ . On allowing the air to enter, the solid instantly melts and more liquid air is formed. The same experiment may be repeated many times by simply opening and shutting the stopcocks. When the liquid air loses too much nitrogen, then it no longer solidifies. This apparatus may be used to show that when liquid air is running freely into B, liquefaction is instantly arrested by allowing hydrogen to enter instead of air.

*Samples of Air Liquefied in Sealed Flasks.*—In a paper "On the relative behaviour of chemically prepared and of atmospheric nitrogen," communicated to the Chemical Society in December 1894, the plan of manipulating such samples was described. The arrangement shown in Fig. 4 illustrates how oxygen in A under 0.21 of an atmos. pressure, and nitrogen in B under 0.79 of an atmos., can be compared as to the first appearance of liquefaction in each, and finally as to their respective tensions when the temperature is as low as that of solid nitrogen. The flasks A and B have a capacity of more than a litre. Each has a manometer sealed on, and in each phosphoric anhydride is inserted to secure dryness. A large vacuum vessel C holds the liquid air, which is gradually lowered in temperature by boiling under exhaustion. The moment liquefaction takes place, the tubes D', D" begin to show liquid. These tubes must be drawn fine at the end when accurate observations are being made. In the same manner two oxygen flasks were compared. One filled with gas made from fused chlorate of potash, contained in a side tube sealed on to the flask. The other was treated in the same way, only the chlorate had a little peroxide of manganese added. The former gave perfectly clear blue liquid oxygen, the latter was turbid from solid chlorine. Two flasks of dry air that had stood over phosphoric anhydride were liquefied side by side, the only difference between the samples being that one was free from carbonic acid. The one gave a liquid that was perfectly clear, the other was turbid from the 0.01 per cent. of carbon dioxide.

The temperature was lowered by exhaustion until samples of liquid air from two flasks placed side by side as in Fig. 4 became solid. The flasks were then sealed off for the purpose of examining the composition of the air that had not been condensed. The one sample contained oxygen, 21.19 per cent., and the other 20.7 per cent. This is an additional proof to the one previously given that, substantially, the oxygen and nitrogen in air liquefy simultaneously, even under gradually diminishing pressure, and that in these experiments all the known constituents of air are condensed together. These results finally disprove the view expressed in 'A System of Inorganic Chemistry,'\* by Professor Ramsay, where he says: "Air has been liquefied by cooling to  $-192^{\circ}$ , but as oxygen and nitrogen have not the same boiling points, the less volatile oxygen doubtless liquefies first." My old experiments† showed that the substance now known as argon became solid before nitrogen, but chemical

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\* 1891, p. 70.

† See Proc. Chem. Soc. Dec. 1894.

nitrogen and air nitrogen, with its 0·1 per cent. of argon, behaved in substantially the same way on liquefaction.

*Liquid Nitric Oxide.*—Great interest attaches to the behaviour of nitric oxide at low temperatures. Professor Olszewski has examined the liquid and describes it as colourless. Samples of nitric oxide have been prepared in different ways. These have been transferred to liquefaction flasks, where they were left in contact with anhydrous potash, sulphuric acid alone, a mixture of sulphate of aniline and sulphuric acid, or phosphoric acid, for many days before use. Each of the samples, when cooled, gave a nearly white solid, melting into a *blue liquid*. The colour is more marked at the melting point than at the boiling point. Liquid nitric oxide is not magnetic; neither is the solid phosphorescent. Colour in the oxides of nitrogen evidently begins with the second oxide. Solid nitric oxide does not show any chemical action when placed in contact with liquid oxygen, provided the tube containing it is completely immersed; but if the tube full of liquid oxygen is lifted into the air, almost instantly a violent explosion takes place.

*Specific Gravities taken in Liquid Oxygen.*—In a good vacuum vessel specific gravities may be taken in liquid oxygen with as great ease as in water. The shape of the vacuum vessel which works best is shown in Fig. 4. It must contain excess of mercury and be thoroughly boiled out, so that the inner vessel becomes completely coated with a mercury mirror as soon as the liquid oxygen is filled in. Instead of a mercury vacuum, the interior may be silvered and highly exhausted by a Sprengel pump. The flasks must also be thoroughly clean and free from dust, otherwise the liquid oxygen will not remain tranquil. Any superheating is prevented by inserting a long narrow piece of wood for a moment before the final weighing.

Some twenty substances were weighed in liquid oxygen,\* and the apparent relative density of the oxygen determined. The results were then corrected, using Fizeau's values for the variation of the coefficient of expansion of the solids employed, and thereby the real density of liquid oxygen calculated. The resulting value was 1·1375, bar. 766·5, in the case of such different substances as cadmium, silver, lead, copper, silver iodide, calc-spar, rock crystal. The following table gives some of the observations:—

Mean Cubical Coefficient of Expansion between 15° C.—183° C.		Apparent Density of Liquid Oxygen	Real Density of Liquid Oxygen
Cadmium,	$7986 \times 10^{-5}$ .. ..	1 1188	1·1359
Lead,	7892 „ .. ..	1·1197	1·1367
Copper,	4266 „ .. ..	1 1278	1 1470
Silver,	5185 „ .. ..	1·1278	1 1385
Calc-spar,	1123 „ .. ..	1 1352	1·1376
Rock crystal,	2769 „ .. ..	1·1316	1·1376
Silver Iodide,	0189 „ .. ..	1·1372	1·1376

\* The liquid oxygen might possibly contain a small proportion of nitrogen.

Direct determinations with an exhausted glass cylindrical vessel displacing about 22 c.c. gave 1.1378. Fizeau's parabolic law for the variation of the coefficient of expansion holds down to  $-183^{\circ}$ . The solid which showed the greatest contraction was a block of compressed iodine; the one that contracted least being a compressed cylinder of silver iodide. Wroblewski gave the density of liquid oxygen at the boiling point as 1.168, whereas Olszewski found 1.124. The variation of density is about  $\pm 0.0012$ , for 20 mm. barometric pressure. Much work requires to be done in the accurate determination of the physical constants of liquid gases.

*Liquid Air.*—A large silver ball weighed in liquid air gave the density of the latter as 0.910, and the corresponding density of nitrogen at its boiling point 0.850. It is difficult to be quite certain that the constituents of liquid air are in the same proportion as the gaseous ones, so that further experiments must be made. Liquid air kept in a silvered vacuum vessel gradually rises in boiling point from the instant of its collection, the rate of increase during the first hour being nearly directly proportional to the time. As the increase amounted to  $1^{\circ}$  in ten minutes, the boiling point of oxygen ought to have been reached within two hours. The density of liquid air, however, does not reach that of pure oxygen even after thirty hours' storage. The large apparatus of the Royal Institution for air liquefaction can be arranged to deliver liquid air containing 49 per cent. of oxygen, which gives off gas containing 20 per cent. of oxygen, rising after six hours to 72.6 per cent.

*Combustion in Liquid Oxygen.*—A small ignited jet of hydrogen burns continuously below the surface of liquid oxygen, all the water produced being carried away as snow. There is a considerable amount of ozone formed, which concentrates as the liquid oxygen evaporates. In the same way graphite or diamond, when properly ignited, burns continuously on the surface of liquid oxygen, producing solid carbonic acid and generating ozone. If liquid oxygen is absorbed in wood charcoal, or cotton-wool, and a part of the body heated to redness, combustion can start with explosive violence.

*Gas Jets containing Liquid.*—The experiments of Joule and Thomson and Regnault on the temperature of gas jets issuing under low pressures are well known. The following observations refer to the pressure required to produce a lowering of temperature sufficient to yield liquid in the gas jet.

The apparatus used in the study of highly compressed gas jets is represented in Fig. 2; where C is a vacuum tube which holds a coil of pipe about 5 mm. in diameter surrounded with carbon dioxide or liquid air for cooling the gas before expansion, and A is a small hole in the silver or copper tube about  $\frac{1}{2}$  mm. in diameter, which takes the place of a stopcock. When carbon dioxide gas at a pressure of 30 or 40 atmos. is expanded through such an aperture, liquid can be seen where the jet impinges on the wall of the vacuum tube, along with a considerable amount of solid. If oxygen gas escapes from the small hole at the pressure of 100 atmos. having been cooled previously

to  $-79^{\circ}$  in the vessel C, a liquid jet is just visible. It is interesting to note, in passing, that Pictet could get no liquid oxygen jet below 270 atmos. This was due to his stopcock being massive and outside the refrigerator. If the oxygen is replaced by air, no liquid jet can be seen until the pressure is 180 atmos., but on raising the pressure to 300 atmos. the liquid air collected well from the simple nozzle. If the carbon dioxide is cooled by exhaustion (to about 1 inch pressure) or  $-115^{\circ}$ , then liquid air can easily be collected in the small vacuum vessel D, or if the air pressure is raised above 200 atmos., keeping the cooling at  $-79^{\circ}$  as before.\* The chief difficulty is in collecting the liquid, owing to the rapid current of gas. The amount of liquid in the gas jet is small, and its collection is greatly facilitated by directing the spray on a part of the metallic tube above the little hole, or by increasing the resistance to the escaping gas by placing some few turns of the tube, like B in the figure, in the upper portion of the vacuum tube, or generally by pushing in more tube in any form. A vacuum vessel shaped like an egg-glass also works well. This practically economises the cool gas which is escaping to reduce the temperature of the gas before expansion, or, in other words, it is the cold regenerative principle. Coleman pointed out long ago that his air machine could be adapted to deliver air at as low a temperature as has yet been produced in physical research. Both Solvay and Linde have taken patents for the production of liquid air by the application of cold regeneration, but the latter has the credit of having succeeded in constructing an industrial apparatus that is lowered in temperature to  $-140^{\circ}$ , or to the critical point of air, in about 15 hours, and from which liquid air containing 70 per cent. oxygen is collected after that time.

For better isolation, the pipe can be rolled between two vacuum tubes, the outer one being about 9 inches long and  $1\frac{1}{2}$  inch diameter, as shown in Fig. 3. The aperture in the metal pipe has a little piece of glass tube over it, which helps the collection of the liquid. With such a simple apparatus, and an air supply at 200 atmos. with no previous cooling, liquid air begins to collect *in about five minutes*, but the liquid jet can be seen in between two and three minutes. It is not advisable to work below 100 atmos.

In Fig. 4 the metallic tube in the vacuum vessel is placed in horizontal rings, leaving a central tube to allow the glass tube C to pass, which is used to cool bodies or examine gases under compression. The inner tube can be filled for an inch with liquid air under a pressure of 60 atmos. in about three minutes. Generally, in the experiments, about  $\frac{1}{2}$  to 4 cubic feet of air passes through the different sized needle holes per minute when the pressure is about 200 atmos. As the small hole is apt to get stopped, for general

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\* The liquefaction is taking place in this condition at  $1\frac{1}{2}$  times the critical temperature.\* Hydrogen similarly expanded at the melting point of air ( $-214^{\circ}$  C.) behaves exactly in the same way.



working it is better to use a needle stopcock, worked from the outside by a screw passing through the middle of the coil of pipe.

In testing the individual coils as to the amount of air passed per minute under different pressures, the arrangement of apparatus shown in the Plate 7 was used.

A is a bottle of compressed air, to which the copper pipe B is attached. This coiled pipe first passes through the vessel C containing water, in order to equalise the temperature, and then through the cork D into the glass vacuum vessel E, when it is led by a large number of convolutions to the bottom, terminating in a minute pin-hole valve F. The released air passes from F right up through the coils and out of the vent by the copper tube G, which in its turn passes through a vessel H similar in its object to C, and is then conducted to a measuring meter Z J.

The following table gives the results of a series of experiments made on one coil as to the rate of discharge of air at different pressures:—

Pressure in Atmospheres	Cubic Feet per Minute Measured under Atmosphere at 15°.
55	0.22
105	0.42
155	0.63
198	0.79
210	0.84
250	1.00
287	1.15
290	1.18

The results show that the rate of air discharge through a fine aperture is directly proportionate to the pressure, or the velocity with which the gas on the high-pressure side enters the orifice, is independent of the density. Actual measurements of the size of the needle-hole resulted in proving that the real velocity of the air entering the aperture on the high-pressure side was about 500 feet per second. In all these experiments the temperature of the coil was not allowed to get so low as to produce any visible trace of condensation in the air jet. Just before liquefaction the rate of discharge of air through the same aperture may be doubled, the pressure remaining steady, owing to change in the viscosity of the gas and other actions taking place at low temperatures. The above measurements can only be regarded as representing the general working of such regenerating coils.

A double coil of pipe has advantages in the conduct of some experiments. The efficiency is small, not exceeding the liquefaction of 2 to 5 per cent. of the air passing, but it is a quick method of

reaching low temperatures, and easy to use for cooling tubes and collecting a few hundred c.c. of liquid air, especially if the compressed air is delivered at the temperature of  $-79^{\circ}$  before expansion. With larger vacuum vessels and larger regenerating coils no doubt the yield of liquid could be increased. The liquid air resulting from the use of this form of apparatus contains about 50 per cent. of oxygen. If the air is cooled with solid carbonic acid previous to its reaching the vacuum tube coil of pipe, the only change is to reduce the percentage of oxygen to 40. Successive samples of liquid taken during the working had nearly the same composition. If the arrangement shown in Fig. 2 is used, with silver tube, about  $\frac{1}{16}$  inch bore, and a foot or two coiled in upper part of the vacuum vessel, liquid air containing 25 per cent. of oxygen is obtained. On the other hand, the percentage of oxygen can be increased by a slight change in the mode of working.

In the above experiments air is taken at the ordinary temperature, which is a little above twice its critical temperature, and is partially transformed in a period of time which, in my experiments, has never exceeded ten minutes, simply and expeditiously into the liquid state at its boiling point,  $-194^{\circ}$ , or a fall of more than  $200^{\circ}$  has been effected in this short period of time.

*Experiments on Hydrogen.*—Wroblewski made the first conclusive experiments on the liquefaction of hydrogen in January 1884. He found that the gas cooled in a tube to the boiling point of oxygen, and expanded quickly from 100 to 1 atmos., showed the same appearance of sudden ebullition as Cailliet had seen in his early oxygen experiments. No sooner had the announcement been made than Olszewski confirmed the result by expanding hydrogen from 190 atmos. previously cooled with oxygen and nitrogen boiling in vacuo. Olszewski declared in 1884 that he saw colourless drops, and by partial expansion to 40 atmos. the liquid hydrogen was seen by him running down the tube. Wroblewski could not confirm these results, his hydrogen being always what he called a "liquide dynamique." He proposed to get "static" liquid hydrogen by the use of hydrogen gas as a cooling agent. Professor Ramsay, in his 'System of Inorganic Chemistry,' published long after the early experiments of Pictet, Cailliet, Wroblewski and Olszewski on the liquefaction of hydrogen had been made, sums up the position of the hydrogen question in 1891 as follows (p. 28):—"It has never been condensed to the solid or liquid states. Cailliet, and also Pictet, who claim to have condensed it by cooling it to a very low temperature, and at the same time strongly compressing it, had in their hands impure gas. Its critical temperature, above which it cannot appear as liquid, is probably not above  $-230^{\circ}$ ." It has to be remembered that 7 per cent. of air by volume in hydrogen means about 50 per cent. by weight of the mixed gases. Even 1 per cent. by volume in hydrogen is equivalent to some 13 per cent. by weight.

The following table gives the theoretical temperatures reached for

an instant during the adiabatic expansion of hydrogen under different conditions:—

Initial Pressure Atmospheres.	Initial Temperature.	Theoretical Final Temperature (Absolute).
500 (Pictet) .. .. .	—130°	25°
300 (Chaillet) .. .. .	0	52
100 (Wroblewski) .. .. .	—184	24
180 (Olszewski) .. .. .	—210	14
100 .. .. .	—200	19·5
200 .. .. .	—200	15·7
500 .. .. .	—200	12·7

The calculations show that little is gained by the use of high pressures. The important inference to be drawn from the figures is to start with as low a temperature as possible.

From 1884 until his death, in the year 1888, Wroblewski devoted his time to a laborious research on the isothermals of hydrogen at low temperatures. The data thus arrived at enabled him, by the use of Van der Waal's formulæ, to define the critical constants of hydrogen, its boiling point, density, &c., and the subsequent experiments of Olszewski have simply confirmed the general accuracy of Wroblewski's results. Wroblewski's critical constants of hydrogen are given in the following table:—

Critical temperature .. .. .	— 210°
„ pressure .. .. .	13·3 atmos.
„ density .. .. .	0·027
Boiling point .. .. .	— 250°
Density at boiling point* .. .. .	0·003

In a paper published in the *Phil. Mag.* September 1884, "On the Liquefaction of Oxygen and the Critical Volumes of Fluids," the suggestion was made that the critical pressure of hydrogen was wrong, and that instead of being 99 atmos. (as deduced by Sarrau from Amagat's isothermals) the gas had probably an abnormally low value for this constant. This view was substantially confirmed by Wroblewski finding a critical pressure of 13·3 atmos., or about one-fourth that of oxygen. The 'Chemical News' (September 7, 1894) contains an account of the stage the author's hydrogen experiments had reached at that date. The object was to collect liquid hydrogen at its boiling point in an open vacuum vessel, which is a much more difficult problem than seeing the liquid in a glass tube under pressure and at a higher temperature. In order to raise the critical point of hydrogen to about — 200°, from 2 to 5 per cent. of nitrogen or air was mixed with it. This is simply making an artificial gas containing a large

\* It is probable that the real density of boiling liquid hydrogen may lie between 0·12 and 0·18.

proportion of hydrogen, which is capable of liquefaction by the use of liquid air. The results are summed up in the following extract from the paper :—"One thing can, however, be proved by the use of the gaseous mixture of hydrogen and nitrogen, viz. that by subjecting it to a high compression at a temperature of  $-200^{\circ}$ , and expanding the resulting liquid into air, a much lower temperature than anything that has been recorded up to the present time can be reached. This is proved by the fact that such a mixed gas gives, under the conditions, a paste or jelly of solid nitrogen, evidently giving off hydrogen because the gas coming off burns fiercely. Even when hydrogen containing only some 2 to 5 per cent. of air is similarly treated the result is a white, solid matter (solid air) along with a clear liquid of low density, which is so exceedingly volatile that no known device for collecting has been successful." \*

In Professor Olszewski's paper "On the Liquefaction of Gas," † after detailing the results of his hydrogen experiments, he says :—"The reason for which it has not hitherto been possible to liquefy hydrogen in a static state is, that there exists no gas having a density between that of hydrogen and nitrogen, and which might be, for instance, 7–10 ( $H = 1$ ). Such a gas would be liquefied by means of liquid oxygen or air as cooling agent, and afterwards used as a recognised menstruum in the liquefaction of hydrogen. Science will probably have to wait a very long time before this suggestion of how to get "static" liquid hydrogen is realised. The proposal Wroblewski made in 1884 of using the expansion of hydrogen as a cooling agent to effect the change of state, is far more direct and practicable.

*Liquid Hydrogen Jet and Solid Hydrogen.*—Hydrogen, cooled to  $-191^{\circ}$  ( $80^{\circ}$  abst.  $t$ ), the boiling point of air, is still at a temperature which is two and a half times its critical temperature, and its direct liquefaction at this point would be comparable to that of air taken at  $60^{\circ}$ , and liquefied by the apparatus just described. In other words, it is more difficult to liquefy hydrogen (assuming it to be supplied at the temperature of boiling air) than it is to produce liquid air starting from the ordinary atmospheric conditions. Now, air supplied at such a high temperature greatly increases the difficulty and the time required for liquefaction. Still it can be done, even with the air supply at  $100^{\circ}$ , in the course of seven minutes, and this is the best proof that hydrogen, if placed under really analogous conditions, namely at  $-194^{\circ}$  must also liquefy with the same form of apparatus. It is almost needless to say that hydrogen under high compression at the temperature of  $15^{\circ}$  C. passed through such a regenerating coil, produced no lowering of temperature. Hydrogen cooled to  $-200^{\circ}$  was forced through a fine nozzle under 140 atmos. pressure, and yet

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\* The compressed gas mixture at above  $-210^{\circ}$  was expanded into a large cooled vacuum vessel.

† Phil. Mag. 1895.

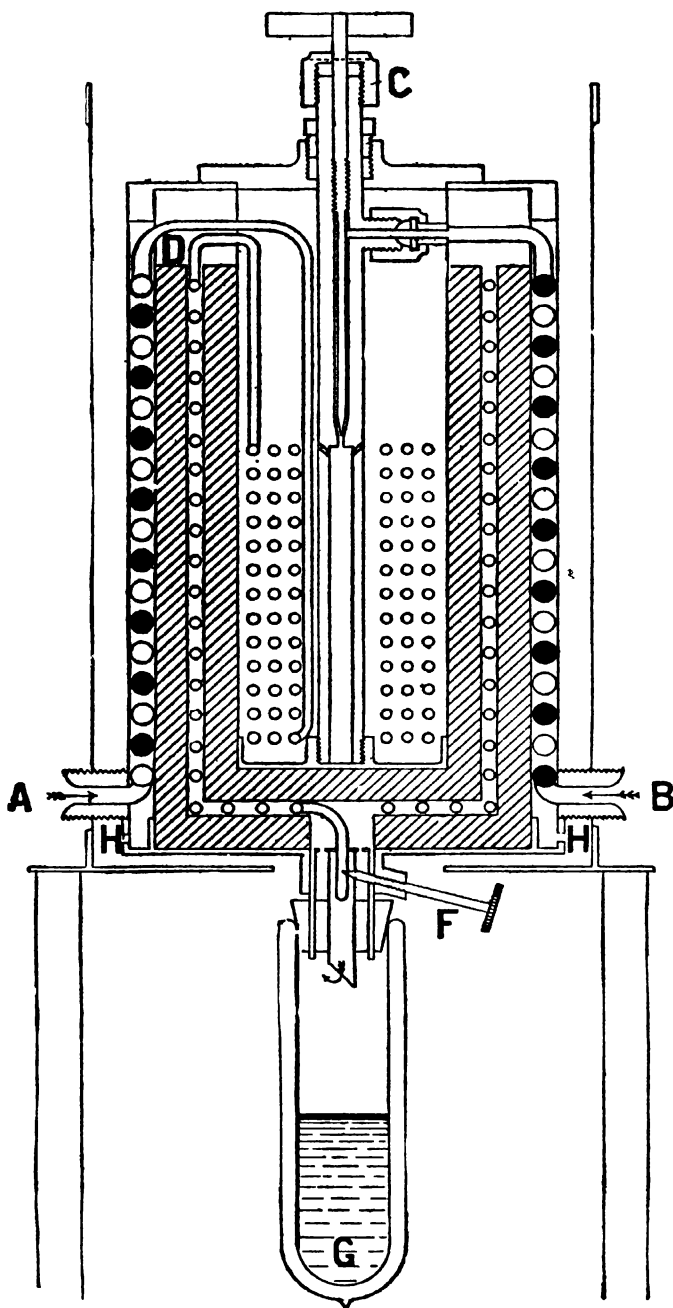


FIG. 1.

LABORATORY LIQUEFACTION APPARATUS FOR THE PRODUCTION OF LIQUID OXYGEN, &c.

A, air or oxygen inlet; B, carbon dioxide inlet; C, carbon dioxide valve.  
D, regenerator coils, F, air or oxygen expansion valve, G, vacuum vessel  
with liquid oxygen. H, carbon dioxide and an outlet, O, air coil, ●, carbon  
dioxide coil.

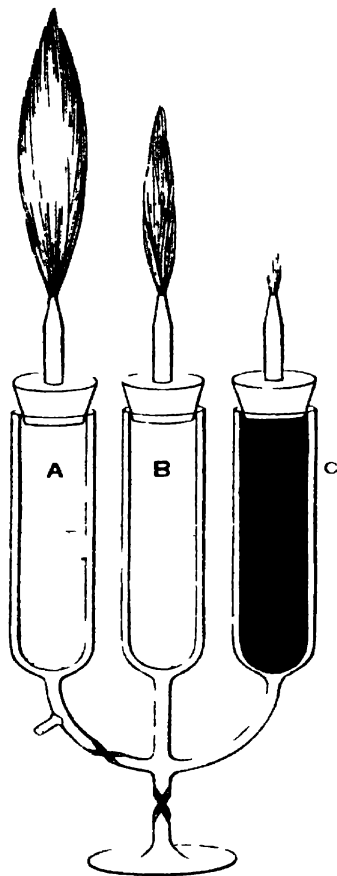


FIG 2

LIQUID ETHYLENE FLAME CALORIMETER

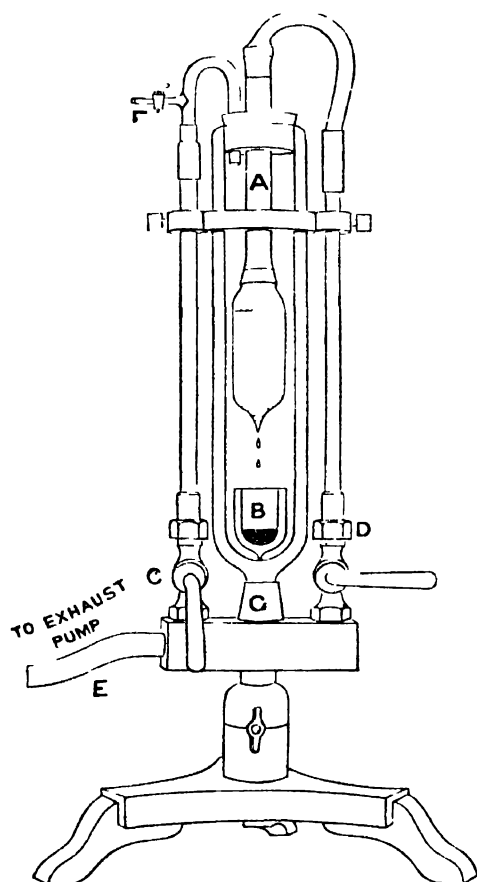


FIG. 3

LECTURE APPARATUS FOR PROTECTING THE LIQUEFACTION OF AIR  
AT ATMOSPHERIC PRESSURE, AND ITS SOLIDIFICATION

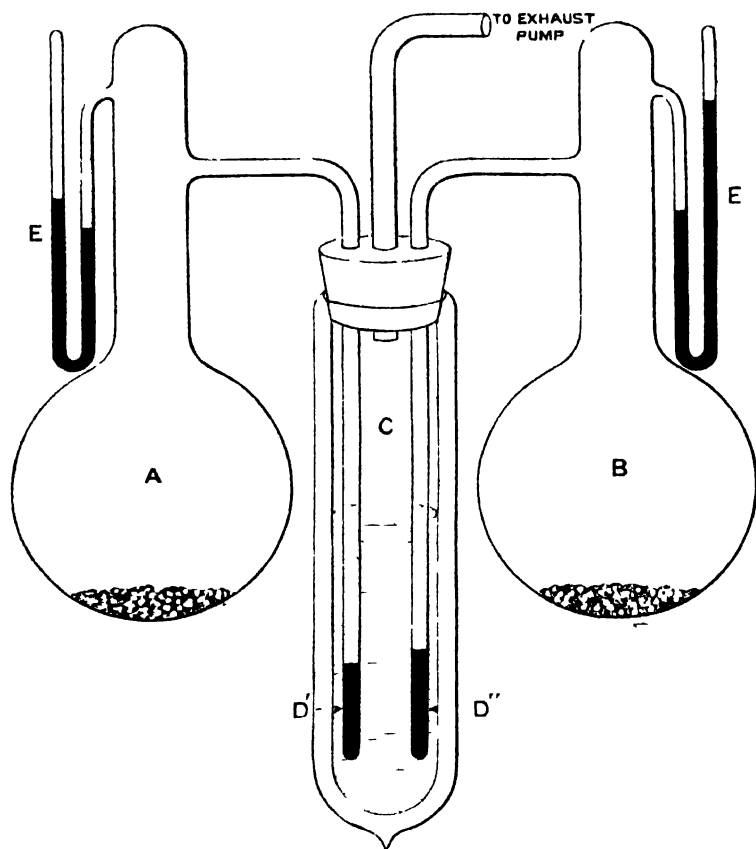


FIG. 1.

PLAN OF COMPARING RELATIVE TEMPERATURES OF LIQUEFACTION AND  
SMALL VAPOUR PRESSURES



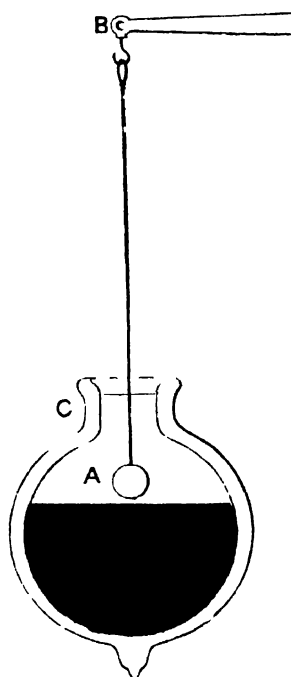


FIG. 5

SPECIFIC GRAVITY VACUUM GLOBE

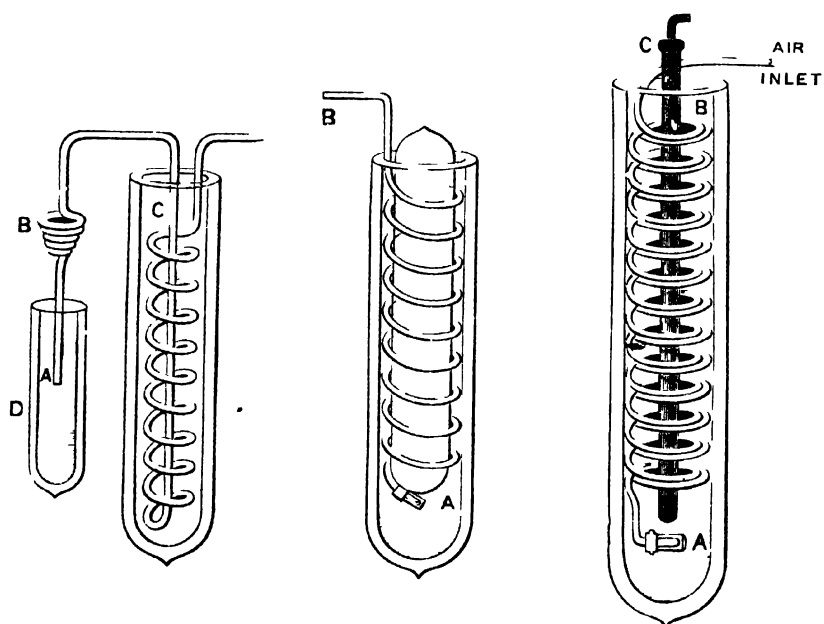


FIG 6

DIFFERENT ARRANGEMENTS OF REGENERATING COILS

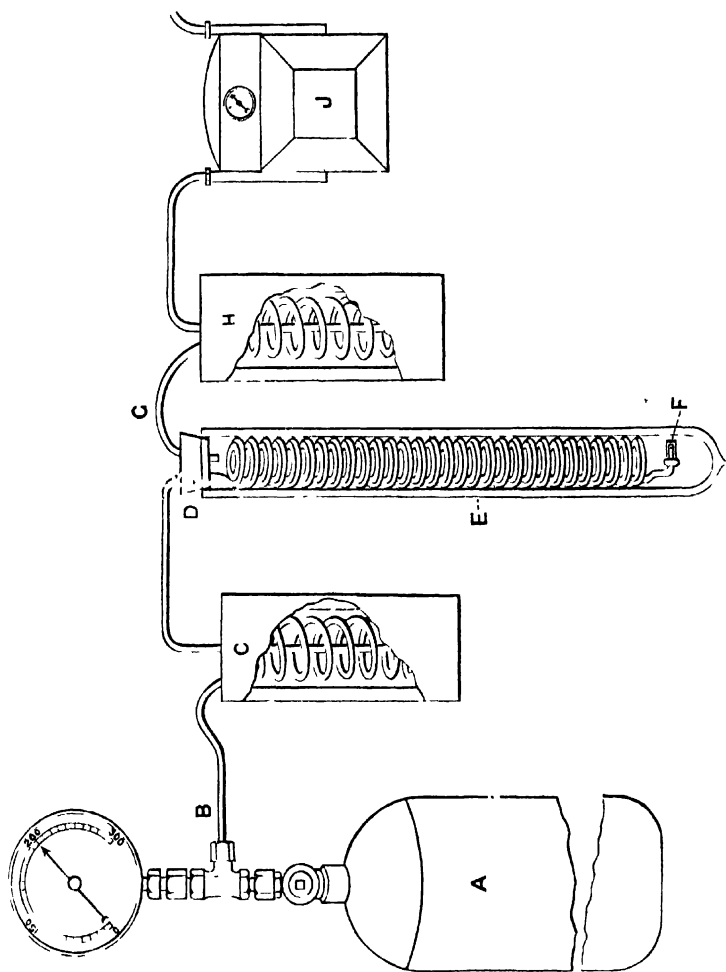


Fig 7

PLAN OF APPARATUS USED IN MEASURING RATE OF PASSAGE OF GAS AT HIGH PRESSURE  
THROUGH A SMALL PIN HOLE

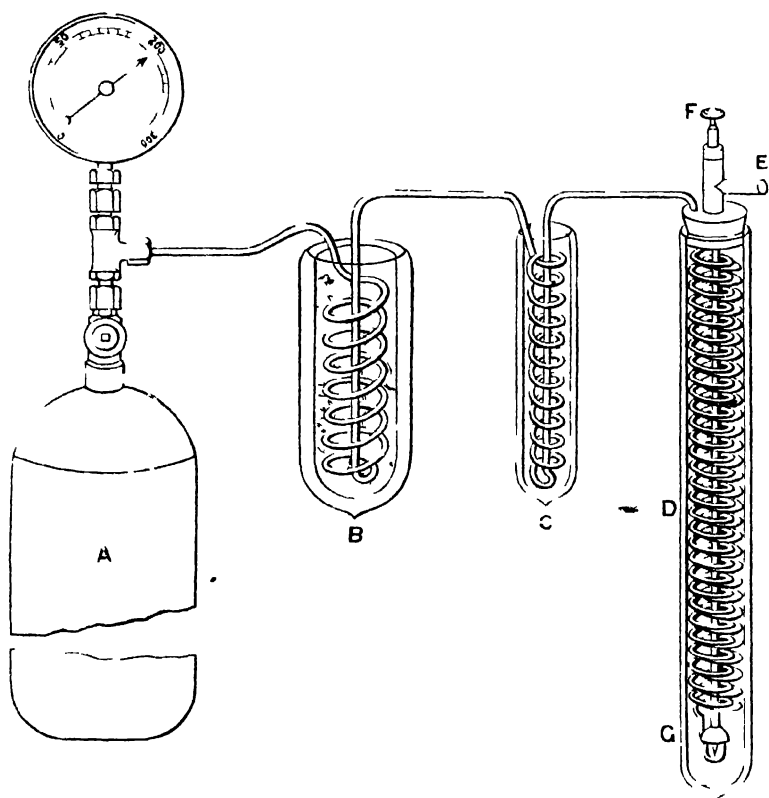


FIG. 8

APPARATUS USED IN THE PRODUCTION OF THE LIQUID HYDROGEN JET

no liquid jet could be seen. If the hydrogen contained a few per cent. of oxygen the gas jet was visible, and the liquid collected, which was chiefly oxygen, contained hydrogen in solution, the gas given off for some time being explosive.

If, however, hydrogen, cooled by a bath of boiling air, is allowed to expand at 200 atmos. over a regenerative coil previously cooled to the same temperature, and similar in construction to that shown in Fig. 8,\* a liquid jet can be seen after the circulation has continued for a few minutes, along with a liquid which is in rapid rotation in the lower part of the vacuum vessel. The liquid did not accumulate, owing to its low specific gravity and the rapid current of gas. These difficulties will be overcome by the use of a differently shaped vacuum vessel, and by better isolation. That liquid hydrogen can be collected and manipulated in vacuum vessels of proper construction cannot be doubted. The liquid jet can be used in the meantime (until special apparatus is completed for its collection) as a cooling agent, like the spray of liquid air obtained under similar circumstances, and this being practicable, the only difficulty is one of expense. In order to test, in the first instance, what the hydrogen jet could do in the production of lower temperatures, liquid air and oxygen were placed in the lower part of the vacuum tube just covering the jet. The result was that in a few minutes about 50 c.c. of the respective liquids were transformed into hard white solids resembling avalanche snow, quite different in appearance from the jelly-like mass of solid air got by the use of the air pump. The solid oxygen had a pale, bluish colour, showing by reflection all the absorption bands of the liquid. The temperatures reached, and other matters, will be dealt with in a separate communication. When the hydrogen jet was produced under the surface of liquid air, the upper part of the fluid seemed to become specifically lighter, as a well marked line of separation could be seen travelling downwards. This appearance is no doubt due in part to the greater volatility of the nitrogen and the considerable difference in density between liquid oxygen and nitrogen. In a short time solid pieces of air floated about, and the liquid subsequently falling below the level of the jet, hydrogen now issued into a gaseous atmosphere containing air, which froze solid all round the jet. There is no reason why a spray of liquid hydrogen at its boiling point in an open vacuum vessel should not be used as a cooling agent, in order to study the properties of matter at some 20° or 30° above the absolute zero.

*Fluorine.*—This is the only widely distributed element that has not been liquefied. Some years ago Wallach and Honsler pointed out that an examination of the boiling points of substituted halogen organic compounds led to the conclusion that, although the atomic weight of fluorine is nineteen times that of hydrogen, yet it must in the

\* In the figure, A represents one of the hydrogen cylinders; B and C, vacuum vessels containing carbonic acid under exhaustion and liquid air respectively; D, regenerating coil; G, pin-hole nozzle; F, valve.

free state approach hydrogen in volatility. This view is confirmed by the atomic refraction which Gladstone showed was 0·8 that of hydrogen, and from which we may infer that the critical pressure of fluorine is relatively small like hydrogen.\* If the chemical energy of fluorine at low temperatures is abolished like that of other active substances, then some kind of glass or other transparent material could be employed in the form of a tube, and its liquefaction achieved by the use of hydrogen as a cooling agent. In any case a platinum vessel could be arranged to test whether fluorine resists being liquefied at the temperature of solid air, and this simple experiment, even if the result was negative, would be of some importance.

During the conduct of these investigations, I have gratefully to acknowledge the able assistance rendered by Mr. Robert Lennox, my chief assistant. Valuable help has also been given by Mr. J. W. Heath.

[J. D.]

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\* On the other hand, the exceptionally small refractivity value observed by Lord Rayleigh in the case of helium shows that the critical pressure of this body is proportionately high. It would therefore be more difficult to liquefy than a substance having about the same critical temperature, but possessing a lower critical pressure, like hydrogen.

Friday, April 17, 1896.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and  
Vice-President, in the Chair.

PROFESSOR G. LIPPMANN, Membre de l'Institut (France).

### *Colour Photography.*

THE problem of colour photography is as old as photography itself. The desire of fixing the colours as well as the design of the beautiful image thrown on the screen of the camera, very naturally occurred to the earliest observers. Since the beginning of this century three distinct solutions of the problem have been realised.

The first solution, not quite a complete one, is founded on the peculiar properties of a silver compound, the violet subchloride of silver. E. Becquerel (1860) converted the surface of a daguerreotype plate into this silver compound, and by projecting on it the image of the solar spectrum, and other objects, obtained good coloured impressions. Poitevin substituted paper for the silver plate as a substratum. No other substance has been discovered that can play the part of the subchloride of silver. Moreover the image is not fixed, in the photographic sense of the word; that is, the coloured impression is retained for any length of time in the dark, but it is blotted out by the action of daylight. The reason of it is this: the Becquerel images are formed by coloured silver compounds, which remain sensible to light; so that they are destroyed by the continued action of light, in virtue of the same action which gave them birth. Despite the numerous experiments made by Becquerel, Poitevin, Zenker and others, no substance has been found that is capable of destroying the sensibility of the subchloride for light without at the same time destroying its colour.

The second method for colour photography is an indirect one, and may be called the three-colour method. It was invented in France by Ch. Cros, and at the same time by M. Ducos du Hauron (1869). German authorities claim the priority of the idea for Baron Bonstetten. Three separate negatives (colourless) are taken of an object through three coloured screens. From these three positives (equally colourless) are made; and, lastly, the colour is supplied to these positives by means of aniline dyes or coloured inks. Thus three coloured monochromatic positives are obtained, which by superposition give a coloured image of the model. In the ingenious process lately invented by Prof. Joly, the three negatives, and appa-

rently the corresponding three positives, are obtained interwoven on one and the same plate. The three-coloured method can give a very good approximation to the truth, and has probably a great future before it. We may call it, nevertheless, an indirect method, since the colours are not generated by the action of light, but are later supplied by the application of aniline dyes or other pigments. Moreover, the choice of these pigments, as well as of the coloured screens through which the negatives have been obtained, is in some degree an arbitrary choice.

The third and latest method by which colour photography has been realised is the interferential method, which I published in 1891, and the results of which I beg to lay before you this evening. It gives fixed images, the colours of which are due to the direct action of the luminous rays.

For obtaining coloured photographs by this method, only two conditions are to be fulfilled. We want (1) a transparent grainless photographic film of any kind, capable of giving a colourless fixed image by the usual means; and (2) we want a metallic mirror, placed in immediate contact with the film during the time of exposition.

A mirror is easily formed by means of mercury. The photographic plate being first enclosed in a camera slide, a quantity of mercury is allowed to flow in behind the plate from this small reservoir, which is connected with the slide by a piece of india-rubber tubing.\* The slide is then adapted to the camera, and the action of light allowed to take place. After exposure the slide is separated from the camera, the mercury reservoir lowered so as to allow the mercury to flow back into it; the photographic plate is then taken out, developed and fixed. When dry, and examined by reflected light, it appears brilliantly coloured.

The sensitive film may be made either of chloride, iodide or bromide of silver, contained in a substratum either of albumen, collodion or gelatine. The corresponding developers, either acid or alkaline, have to be applied; the fixation may be cyanide or bromide of potassium. All these processes I have tried with success. For instance, the photograph of the electric spectrum now projected before your eyes, has been made on a layer of gelatino-bromide of silver, developed with amidol, and fixed with cyanide of potassium.

As you see, bright colour photographs may be obtained without changing the technique of ordinary photography: the same films, developers and fixators have to be employed; even the secondary operations of intensification and of isochromatisation are made use of with full success. The presence of the mirror behind the film during exposure makes the whole difference. From a chemical point of view nothing is changed, the result being a deposit of reduced silver left in the film, a brownish, colourless deposit. And yet the

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\* The glass of the photographic plate has to be turned towards the objective, the film in contact with the metallic mirror



presence of a mirror during exposure causes the colourless deposit to show bright colours. Of course we want to know how this is done; we require to understand the theory of those colours.

We all know that colourless soap-water gives brilliant soap-bubbles; the iridescence of mother-o'-pearl takes birth in colourless carbonate of lime; the gorgeous hues of tropical birds are simply reflected from the brownish substance which forms the feathers. Newton discovered the theory of these phenomena, and subjected them to measurement; he invented for the purpose the experiment called by the name of Newton's rings. Newton showed, as you know, that when two parallel reflecting surfaces are separated by a very short interval, and illumined by white light, they reflect only one of the coloured rays which are the constituents of white light. If, for instance, the interval between the reflecting surfaces is only  $\frac{1}{100000}$  of a millimetre, violet rays are alone reflected, the rest being destroyed by interference: that is, the two surfaces send back two reflected rays whose vibrations interfere with one another, so as to destroy every vibration except that which constitutes violet light. If the interval between the reflecting surfaces be augmented to  $\frac{3}{100000}$  millimetre, the destruction of vibration takes place for every vibration except that of red light, which alone remains visible in this case.

If we consider now this photograph of the spectrum, and especially the violet end of the image, we find that this is formed by a deposit of brown reduced silver. In the case of an ordinary photograph, this deposit would simply be a formless cloud of metallic particles; here the cloud has a definite, stratified form; it is divided into a number of thin, equidistant strata, parallel to the surface of the plate, and  $\frac{1}{100000}$  millimetre apart. These act as the reflecting surfaces considered by Newton, and as they are at the proper distances for reflecting violet rays, and these alone, they do reflect violet rays.

The red extremity of the photograph is equally built up of strata which act in a like manner; only their distance intervals here amount to  $\frac{3}{100000}$  millimetre, and that in the proper interval for reflecting red light. The intermediate parts of the spectral image are built up with intermediate values of the interval, and reflect the intermediate parts of the spectrum.

The appearance of colour is therefore due to the regular structure above described, imprinted on the photographic deposit. The next question is—How has this very fine, peculiar and adequate structure been produced?

It is well known that a ray of light may be considered as a regular train of waves propagated through the ether, in the same way as waves on the surface of water. The distance between two following waves is constant, and termed the wave-length; each sort of radiation, each colour of the spectrum, being characterised by a particular value of the wave-length. Now when a ray of light falls on a sensitive film, this train of waves simply rushes through the film with a velocity of about 300,000 kilometres per second; it impresses

the film more or less strongly, but leaves no record of its wave-length, of its particular nature or colour, every trace of its passage being swept out of form by reason of its swift displacement. The impression therefore remains both uniform and colourless. Things change, however, as soon as we pour in mercury behind the plates, or otherwise provide for a mirror being in contact with it. The presence of the *mirror* changes the propagated waves into *standing waves*. The reflected ray is, namely, thrown back on the incident ray, and interferes with its motion, both rays having equal and opposite velocities of propagation. The result is a set of standing waves—that is, of waves surging up and down, each in a fixed plane. Each wave impresses the sensitive film where it stands, thus producing one of these photographic strata above alluded to. The impression is latent, but comes out by photographic development. Of course the distance between two successive strata is the distance between two neighbouring waves; this, theory shows, is exactly half the wave length of the impressing light. In the case of violet, for instance, the wave-length being  $\frac{1}{10000}$  millimetres, half the wave-length in the above-quoted distance of  $\frac{1}{10000}$  millimetres; this, therefore, is at the same time the interval between two standing waves, in the case of violet light the interval between two successive photographic strata, and at last it is the interval required to exist, according to Newton's theory, for the said strata reflecting violet rays, and making these alone apparent when illuminated by white light.

The colours reflected by the film have the same nature and origin as those reflected by soap-bubbles or Newton's rings; they owe their intensity to the great number of reflecting strata. Suppose, for instance, the photographic film to have the thickness of a sheet of paper (one-tenth of a millimetre), the fabric built in it by and for a violet ray is five hundred stories high, the total height making up one-tenth of a millimetre. Lord Rayleigh, in 1887, has proved *a priori* that such a system is specially adapted to reflect the corresponding waves of light.

How are we now to prove that the above theory is really applicable to the colour photograph you have seen? How can we demonstrate that those bright colours are due not to pigments, but to the interference, as in the case of soap-bubbles? We have several ways of proving it.

First of all, we are not bound to the use of a peculiar chemical substance, such as Becquerel's subchloride of silver; we obtain colour with a variety of chemicals. We can, for instance, dispense entirely with the use of a silver salt; a film of gelatine or coagulated albumen impregnated with bichromate of potash, then washed with pure water after exposure, gives a very brilliant image of the spectrum.

Secondly, the colours on the plate are visible only in the direction of specular reflection. The position of the source by which we illumine the photograph being given, we have to put the eye in a corresponding position, so as to catch the regularly reflected rays. In

every other position we see nothing but a colourless negative. Now, as you are aware, the colours of pigments are seen in any direction. By projecting again a photograph of the spectrum, and turning it to and fro, I can show you that the colours are visible only in one direction.

Thirdly, if we change the incidence of the illuminating rays, that is, if we look at the plate first in a normal direction, then more and more slantingly, we find that the colours change with the incidence exactly as they do in the case of soap bubbles, or of Newton's rings; they change according to the same law and for the same reasons. The red end of the spectrum turns successively to orange, yellow, green, blue and violet. The whole system of colours, the image of the spectrum, is seen to move down into the part impressed by the infra-red. This is what we expect to happen with interference colours, and what again we cannot obtain with pigments.

Fourthly, if while looking at the film normally, we suffer it to absorb moisture—this can be done by breathing repeatedly on its surface—we see that the colours again change, but in an order opposite to that above described. Here the blue end of the spectrum is seen to turn gradually green, yellow, orange, red, and finally infra-red, that is, invisible. The spectrum this time seems to move up into the ultra-violet part of the improved film. By suffering the water to evaporate, the whole image moves back into its proper place; this experiment may be repeated any number of times.

The same phenomenon may be obtained with Newton's apparatus, by slowly lifting the lens out of contact with the plane surface. The explanation is the same in both cases. The gelatine swells up when imbibing moisture. If we consider, for instance, the violet of the spectrum, the small intervals between the strata corresponding to violet rays, gradually swell up to the values proper for green, and for red, and for infra-red; green, then red, then infra-red are therefore successively reflected.

We will wet this photograph of the spectrum with water, project it on the screen, and watch the colours coming back in the order prescribed by theory.

It is necessary to use a transparent film, since an opaque one, such as is commonly in use, would hide the mirror from view; the sensitive substance must be grainless, or at least the grains must be much finer than the dimensions of the strata they are intended to form, and therefore wholly invisible. The preparation of transparent layers gave me at first much trouble; I despaired for years to find a proper method for making them. The method, however, is simply this: if the sensitive substance (the silver bromide, for instance) be formed in presence of a sufficient quantity of organic matter, such as albumen, gelatine or collodion, it does not appear as a precipitate; it remains invisible; it is formed, but seems to remain dissolved in the organic substratum. If, for instance, we prepare a film of albumen-iodide in the usual way, only taking care to lessen the proportions of

iodide to half per cent. of the albumen, we get a perfectly transparent plate, adapted to colour photography.

We want now to go a step further. It is very well for physicists to be contented with working on the spectrum, since that contains the elements of every compound colour; but we all desire to be able to photograph other objects than the spectrum—common objects with the most compound colours. We have again but to take theory as a guide, and that tells us that the same process is able to give us either simple or compound colours. We have then to take a transparent and correctly isochromatised film, expose it with its mercury backing, then develop and fix it in the usual way; the plate, after drying, gives a correct coloured image of the objects placed before the camera. Only one exposure, only one operation is necessary for getting an image with every colour complete.

A plausible objection was offered at first to the possibility of photographing a mixture of simple colours. The objection was this: a ray of violet gives rise to a set of strata separated by a given interval; red light produces another set of strata with another interval; if both co-exist, the strata formed by the red are sure to block out here and there the intervals left between the strata formed by the violet. Is it not to be feared that one fabric will be blurred out by the other, and the whole effect marred? The confusion would be still worse if we consider the action of white light, which contains an infinity of simple components; every interval here is sure to be blocked up.

Mathematical analysis, however, shows this objection to be unfounded; we have great complexity, but not confusion. Every compound ray, both coloured and white, is faithfully rendered. As an experimental proof of this, we will project on the screen photographs of very different objects, namely, stained glass windows, landscapes from nature, a portrait made from life, and vases and flowers.

That the colours here observed are due to interference, and not to the presence of pigments, can be shown in the same way as with the spectrum. Here, again, we observe that the colours are visible only in the direction of specular reflection, that they change with the angle of incidence, that they change and disappear by wetting, and reappear by drying. Pigments remain equally visible and unaltered in colour under every incidence. If we attempted to touch up one of our photographs with oil or water-colours, the adulterated place would stand out on a colourless background by merely observing by diffused light. It is therefore impossible either to imitate or touch up a colour photograph made by the above-described interferential method.

[G. L.]

Friday, May 8, 1896.

LUDWIG MOND, Esq. Ph.D. F.R.S. F.C.S. Manager and  
Vice-President, in the Chair.

PROFESSOR SILVANUS P. THOMPSON, D.Sc. F.R.S. M.R.I.

*Electric Shadows and Luminescence.*

THE early days of the year 1896 were marked by the announcement telegraphed from Vienna to the effect that Professor Roentgen, a man whose name though little known outside the world of science was well known and highly esteemed by those who were initiates in physics, had discovered the existence of rays of a new and extraordinary kind. Taking a Crookes tube, excited of course by a proper electric spark, and covering it up within a case of black cardboard, he found it to produce in the surrounding space some entirely unexpected effects. Black cardboard is impervious not only to ordinary light and to radiant heat, but also to all those other known kinds of invisible light beyond the violet end of the spectrum, known as actinic waves, which are such active agents in the production both of fluorescence and of photographic actions. Yet the invisible emanations of the Crookes tube, which passed freely through the opaque cardboard, were found by Roentgen to be capable of revealing their presence in two ways. In the first place he had seen them to project shadows upon a luminescent screen of paper coated with the highly fluorescent substance called platino-cyanide of barium, and in the second place he had been able to photograph these shadows by letting them fall upon an ordinary photographic plate. The discovery was singular. It revealed the existence of a remarkable and hitherto unexpected species of radiation. It added another to the many puzzling phenomena attendant upon the discharge of electricity in vacuo. It proved that something which in the ordinary sense in which those terms are used is neither light nor electricity was generated in the Crookes tube, and passed from it through substances opaque alike to both.

But that which took the imagination of the multitude by storm, and aroused an interest the intensity the like of which has not been known to be aroused by any other scientific discovery in our times, was not the fact that Professor Roentgen had seen luminescent shadows from a Crookes tube, or had obtained a photograph of those

shadows; it was the entirely subsidiary and comparatively unimportant point that to these mysterious radiations flesh is more transparent than bone.

Let me begin by showing you as a first experiment that same fact which Roentgen announced of the production of luminescent shadows by these invisible rays. Before you there stands a Crookes tube, of the most modern kind,\* for this particular purpose. We have here an induction coil † capable of giving 6-inch sparks, with which we can send electric discharges through the tube, illuminating it with its characteristic golden-green glow. I now cover over the tube and exclude all ordinary light, not with a box of black cardboard but with a black velvet cloth. And now in the darkness I am able to show you on a sheet of paper covered with the highly fluorescent platino-cyanide of barium—the well known substance which Roentgen himself was using—the shadows of objects placed behind. See how this sheet shines in the light of the tube transmuting the invisible radiations into visible light. I hold my purse behind the screen—you see the shadow of the metal clasp, and of the metal contents (two coins and a ring), but you see not the shadow of the leather purse itself, for leather is transparent to these rays while metal is opaque. I hold my hand behind and you see—or at least those of you who are within a few yards of me—the shadow of my hand, or rather of the bones of my hand, surrounded by a fainter shadow of the almost transparent flesh.

Now the second fact that Roentgen announced was that these same rays which escape through the opaque covering and excite fluorescence are also capable of taking photographic impressions of the shadows. There is nothing whatever new about this part of the subject: it is the old photography; there is no “new photography.” Here is a common camera back, and here inside—it is a photographic dry-plate—quite a common dry-plate, such as has been known for ten years. This plate is covered with a black card, so that it may not become fogged by the light of the room when I draw the slide. All I have to do is to lay it upon the table below the Crookes tube so as to cast the shadow upon it, and after due exposure develop the plate in the ordinary well-understood way. Now it may be interesting to see the proof of the fact that bone is less transparent than flesh. So, with your permission, I will ask my little daughter to have her hand photographed. (Experiment made.)

At the time of Roentgen's announcement, the exposure required with the Crookes tubes that were then in existence was from twenty minutes to, I think, two or three hours. Very shortly improvements were made; and with these modern tubes one minute is quite suffi-

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\* A Crookes “focus” tube (Jackson pattern), constructed by Messrs. Newton & Co., of Fleet Street, London.

† An Apps coil capable of giving sparks 25 centimetres in length, but on this occasion excited with only 5 cells, giving sparks about 6 inches in length.

cient for an exposure. Indeed one minute is too much for many objects. I have not previously tried this particular tube, though I judge by its appearance that it is in good condition. As soon as the exposure of one minute is over we will have the plate taken into the dark room and developed in the ordinary way; and when it is developed we will have it brought back into this room and put into the lantern, that you may see what has been done.

Now, while we are taking photographs, I may as well take a second to illustrate another point. Roentgen investigated in the most careful and elaborate way the relative transparency of different materials for these mysterious rays. He noticed that wood, and many substances which are opaque to ordinary light, are transparent to these rays; whilst, on the contrary, several substances that are transparent to light, such as calc-spar and heavy glass, are very opaque toward them. Many experimenters have examined this question of relative transparency. I devoted a day or two to the study of gems, and found that imitation rubies made of red glass are much more opaque than real rubies, and that paste diamonds are much more opaque than real diamonds. Real diamonds and rubies are indeed very transparent, and scarcely cast any shadows on the luminescent screen, though I have found diamond to be more opaque than an equal thickness of black carbon. There are laid upon this piece of card two rubies, one being only a glass ruby. There is also a row of four small diamonds. I will leave you to find out whether they are false or real. And then there are three larger diamonds, one of which is uncut and is a genuine South African stone. I lay them down upon a photographic plate and expose them to the Roentgen rays so that we may test their relative transparency. (The two photographs thus taken were projected upon the screen at the close of the lecture.)

Amongst the things which Roentgen told us was the fact that different kinds of glass are unequally transparent: that lead-glass, for instance, is much more opaque than soda-glass, or potash-glass, or, indeed, any glass which does not contain a heavy metal like lead. He found that practically the transparency was governed by the density; that the heavy or the dense substances were the more opaque. There is now some reason to correct that statement, though in the main as a first approximation it is perfectly true. Professor Dewar has shown that you must take into account, not the density in gross but the atomic weight. Taking any homologous series, for example, such as a number of sulphides, or oxides, or chlorides, that one which contains the atomically heavier metal will be the more opaque. Again, the bromide of sodium is more opaque than the chloride of the same metal, and the iodide is more opaque than the bromide. But as the correspondence between relative opacity and molecular or atomic weight breaks down when we try to pass from one series of compounds to a different series, there is some reason to carry the matter to a further degree of approximation. We must go

beyond the suggestion of atomic weight. The nearest approach to a law that I have been able to get at yet, on comparing tables of statistics, is that the transparency is proportional to the specific heat. For homologous series this is, of course, the same as saying that the transparency is inversely proportional to the molecular weight.

Roentgen found all the heavy metals to be remarkably opaque, while light metals like sodium and aluminium, and even zinc, are remarkable for their transparency. Aluminium, which is opaque to every known kind of light, is transparent, even in sheets half an inch thick, to these rays. Lithium, the lightest of solid metals, and with an atomic weight 7 as against aluminium 27, is so transparent that I have not been able yet even to see its shadow. Of all liquids water is the most transparent, and it has the highest specific heat of all of them.

Roentgen further found these rays to be incapable either of refraction by lens or prism,\* or of reflection by any polished mirror. Reflection there is in one sense, that of diffuse reflection, such as white paper exercises on common light. No lens can concentrate these rays: they are also apparently incapable of being polarised. One difficulty in experimenting on these strange properties is that air itself acts as a turbid medium, reflecting back diffusely, as a smoky cloud would do for ordinary light, a portion of the rays.

Finding that these radiations differed in so many ways from ordinary light, and while resembling and even surpassing ultra-violet rays in their strong actinic properties, yet differed entirely from them in respect of the properties of refraction, reflection and polarisation, he named them "X-rays." To judge by his own writing, he appeared to wish that they might prove to be longitudinal vibrations in the ether, the possibility of the existence of which has been a subject of speculation on the part of some of the most learned of mathematical physicists. Others have suggested that these X-rays are transverse vibrations of a much higher frequency and shorter wave length than any known kind of ultra-violet light. Others again see in them evidence that radiant matter (i.e. cathodic streams of particles) can traverse the glass of a Crookes tube, and regard them as material in their nature. Lastly, it has been suggested that they may be neither waves nor streams of matter, but vortex motions in the ether.

To follow out the bearings of these speculations, as well as to trace the development of discovery, let us go back a little and consider what was the starting point of Roentgen's research. He was using a Crookes tube. It is one of the difficulties of my task to-night that I have to speak in the presence of him who is the master of us all in

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\* Perrin in Paris, and Winkelmann in Jena, have independently found what they believe to be evidence of refraction through an aluminium prism. Both observers detected a slight deviation, but in a direction toward the refracting angle, showing aluminium to have for these rays a refractive index slightly less, with respect to air, than unity



this subject of electric discharges in the vacuum tube. But to understand the discoveries of Crookes let us first witness a few experimental illustrations of the phenomena of electric discharges in vacuum tubes. Many of them have been known for half a century. We all know of the researches made in England by Gassiot, and by Varley and others, and the tubes of Geissler of Bonn are a household word. But there is one set of researches which deserves to be known far better than it is, that made by Dr. W. H. Th. Meyer, of Frankfort, whose pamphlet \* I hold in my hand. In it he depicts a number of tubes in various stages of exhaustion, including one in that highest stage of exhaustion which one is prone to think of modern origin.

In order to illustrate the successive phenomena which are produced when electric discharges are sent through a tube during progressively increasing exhaustion, there is here exhibited a set of identical tubes. Each is a simple straight tube, having sealed in at each end an electrode terminating in a short piece of aluminium wire. The electrode by which the electric current enters is known as the anode, that by which it leaves the tube as the kathode. The only difference between these eight tubes lies in the degree of rarefaction of the interior air. The first one contains air at the ordinary pressure. As its electrodes are about 12 inches apart I am unable with the Apps induction coil (excited to throw an 8-inch spark) to send a spark through it. From the second tube about four-fifths of the air has been abstracted, and here we obtain a forked brush-like spark between the electrodes. The third tube has been exhausted to about one-twentieth part, and shows as the discharge a single thin red linear spark like a flexible luminous thread. When, as in the fourth tube, the exhaustion is carried so far as to leave but one-fortieth, the red line is found to have widened out into a luminous band which extends from pole to pole, while a violet mantle makes its appearance at each end and spreads over both of the electrodes. On carrying the exhaustion to the stage shown by the fifth tube, where only about  $\frac{1}{1000}$  of the original air is left behind, we note that the luminous column has broken up transversely into flickering striae, that the violet mantle round the kathode has become more distinct, and is separated by a dark interval from the luminous red column, while a second and very narrow dark space appears to separate the violet mantle from the surface of the kathode. In the sixth tube the exhaustion has been carried to about  $\frac{1}{10000}$ . The flickering striae have changed shape and colour, being paler. The light at the anode has dwindled to a small bright patch. The violet glow surrounding the kathode has expanded to fill the whole of that end of the tube; the dark space has become more distinct, and within it the kathode now shows on its surface an inner mantle of dull red light. There is a slight tendency for the

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\* Beobachtungen über das geschichtete electrische Licht, sowie über den merkwürdigen Einfluss des Magneten auf dasselbe: von Dr. W. H. Theodor Meyer. Berlin, 1858.

glass to show a greenish fluorescence near the kathode end. In the seventh tube the luminous column has subsided into a few greyish-white nebulous patches, the dark space round the kathode has greatly expanded, and the glass of the tube has now begun to show a yellow-green fluorescence. The exhaustion has been pushed so that only about  $\frac{1}{30000}$  or less of the original air is present. In the eighth and last tube only one or two millionths of the original air have been left, with the result that the tube now offers an enormously increased resistance to the passage of the discharge. All the internal flickering nebulosities have vanished; the tube looks as though there were no residual air within. But now the glass itself shines with a fine yellow-green fluorescence which is particularly bright in the region around the kathode. Were the exhaustion to be carried much further the spark from this induction coil would no longer pass, so high would the resistance become. All these successive stages up to the last can be shown in one and the same tube attached to a modern rapid air pump. But for the proper production of the high vacua of the last stages, where electric shadows are alone produced, nothing short of a mercurial pump, either in the form invented by Sprengel or in that used by Geissler (or one of the recent modifications) will suffice.

The phenomenon of fluorescence of the glass, which manifests itself when the exhaustion has become sufficiently high, was known in a general way as far back as 1869 or 1870. The tube next to be shown is a modern reproduction of a tube used at that time by Hittorf, of Munster. It differs from the tubes last shown by having a bend in it. Hittorf observed that when such a tube is exhausted sufficiently highly to give at the kathode the characteristic greenish-yellow fluorescence, this greenish-yellow fluorescence refused to go round the bend. It might appear at one end or the other, according to the direction in which the discharge was being sent, but would not go round the bend. The effect was as if the discharge went in straight lines from the bit of wire that served as kathode to the walls of the tube. Indeed shadow effects were observed by him, and by Wright, of Yale, and afterwards independently by Crookes, who greatly extended our knowledge of the facts. We may take this fact, that the fluorescence caused by the kathode will not go round a corner, as the starting point of the memorable researches of Crookes on radiant matter a score of years ago.

Before you are several tubes which illustrate the researches made by Crookes. The first is a simple glass bulb into which are sealed the two electrodes—the anode, by which the current enters, terminating in a bit of stout aluminium wire; the other, by which the current leaves, called the kathode, terminating in a small flat aluminium disk. The glass bulb was itself highly exhausted—how highly we shall presently see. From the flat front surface of the kathode, when sparks are sent through the bulb, a sort of back-discharge takes place in a direction normal to the surface. This

discharge, which only occurs at a very high degree of exhaustion, possesses several properties which distinguish it from all other kinds of discharge. It is propagated in straight lines, causes a brilliant luminescence wherever it strikes against the glass walls of the tubes, casting shadows of intervening objects, it heats the surface on which it impinges, and strikes them with a distinct mechanical force. Singular to relate, it is also capable of being deflected by a magnet as though it were a flexible conductor carrying the current. Struck by the singularity of these cathode rays or cathode discharges, which formed the subject of several beautiful rescarches, Crookes advanced the hypothesis that they consisted of flights of negatively electrified particles or "radiant matter." The particles he sometimes spoke of as molecules, sometimes as dissociated atoms, or, as we should now say, ions. He studied the wanderings of these flying particles by inserting within the bulb at different points auxiliary electrodes. He found the interior of the bulb to be positively electrified in all parts except within the dark space which surrounds the cathode, that is to say, except within the range of the actual cathode discharge. The cathode discharge itself was found to be possessed, to an extent exceeding any other known agency, of the power of exciting fluorescence and phosphorescence in minerals and gems. The cathode rays were themselves discernible as they crossed the interior of the tube. In such a bulb the cathode rays would form a blue streak impinging straight upon the anode. The cathode used in the next Crookes tube, is of a concave shape. Crookes found that, since the cathode rays left the surface normally, the result of curving the cathode was to focus the rays toward the centre of curvature. By so focussing the rays upon a bit of platinum foil, it was found possible to fuse and even melt the metal.

Unlike the discharges obtained at lower stages of rarefaction, the direction of these cathode rays was found to be independent of the position of the anode. He found cathode rays to be produced even when no internal electrodes were inserted, and when, instead, external patches of tinfoil were attached to the glass. Their mechanical action he studied by causing them to impinge upon the vanes of a pivoted fly which was thereby set into rotation. In a later experiment he caused the fly of a "molecule mill" to be set into rotation, not by the impact of the cathodic discharge but by the kinetic energy of the particles returning back toward the anode after they had impinged against the walls of the tube and lost their negative electric charges. A mere resumé of Crookes's work in those years beginning about 1869 or 1870, and extending not only for ten years actively, but going on at intervals until a year or two ago, would of itself fill a whole course of lectures. Into the controversy which has arisen between Crookes and the English physicists on the one hand and the German physicists on the other, there is no need to enter. Suffice it to say that while the German physicists mostly reject Crookes's hypothesis of radiant matter, and regard all these various phenomena as the

result of mere wave motions within the tube, the British physicists, including Lord Kelvin and Sir George Stokes, accept Crookes's view of the material nature of the kathode rays. Who, indeed, that has seen the molecule mill at work can doubt that, whether vibrations are present or not (and doubtless there are vibrations present), there are actually streams of moving particles as an essential feature of the kathodic discharge? For the moment the victory undoubtedly rests with the views of Crookes.

But of all these phenomena the one which concerns us most is that of the production of electrical shadows. Erecting in the path of the kathode rays an obstacle cut out in sheet metal—a cross of thin aluminium is the favourite object—a shadow of it is observed to be cast upon the wall of the tube behind it; the glass phosphorescing brilliantly except where shielded from the impact of the kathode rays, so that the shadow comes out dark against a luminous background. Common soda-glass gives this greenish-golden tint, while lead-glass exhibits a blue phosphorescence. Not

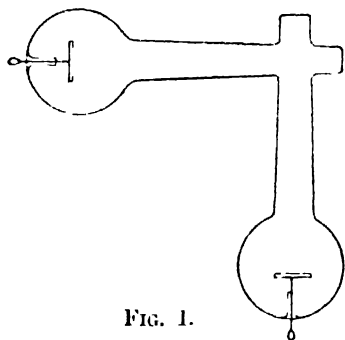


FIG. 1.

glass alone, but diamonds, rubies, emeralds, calc-spar and other earthy materials, such as alumina, and notably yttria, produce the most brilliant effects under the kathode discharge, some of them only fluorescing transiently, others with a persistent phosphorescence. As a sample is shown a tube in which a sea shell, slightly calcined to remove organic matter, is made to emit a brilliant luminescence under the impact of rays from a kathode placed above it. The shell itself casts a shadow against the lower part of the tube. Some of the shadow effects are very mysterious and have recently claimed much of my attention. The size of the kathodic shadows is affected by the electrical state of the object. Electrifying it positively makes its shadow shrink to smaller dimensions. Electrifying it negatively causes a singular enlargement of the shadow. There seems to be no difference between the shadow of a metallic body and that of a non-metallic body of the same size. All bodies cast shadows, however thin. Even a film of glass  $\frac{1}{10000}$  inch thick—so thin that it showed iridescence like a soap bubble—was found by Crookes to cast its shadow.

Another point noticed by Crookes was that if the exhaustion is carried very far, and the tube is stimulated by a sufficiently strong electromotive force, the phosphorescence may occur at points not in the line of discharge but round a corner. Not that the kathode rays turn the corner, however. Apparently some of the more quickly moving or perhaps more highly charged particles—atoms, molecules or ions—those, in fact, described by Crookes as “loose and erratic”—would manage to get round the corners and produce effects of a

more or less directly cathodic kind in places where they could not have penetrated by any motion in a straight line.

Here (Fig. 1) is a tube—a variation on one of Hittorf's, having two branches that cross one another at right angles. There are two small disks of aluminium in the bulbous ends to serve as electrodes. When either of these is made the kathode, the whole limb in which it is situated fluoresces brilliantly of a golden-green tint, particularly at the distant end. But the other limb remains dark, save for a little nebulous blue patch, near the anode, due to residual gas. Another tube (Fig. 2) is made as a zigzag, and here again only the end branch shines. On reversing the current the luminescence shifts to the other end. But when the tube is more highly exhausted, the phosphorescence is observed not only in the end branch where the kathode is, but also slightly at the end wall of each branch of the zigzag. Apparently the residual gas will act partly as its own kathode, and throw off something which causes the glass beyond to phosphoresce.

And now let me remark that not one of all the tubes shown since the first one, is capable of showing a shadow upon the fluorescent

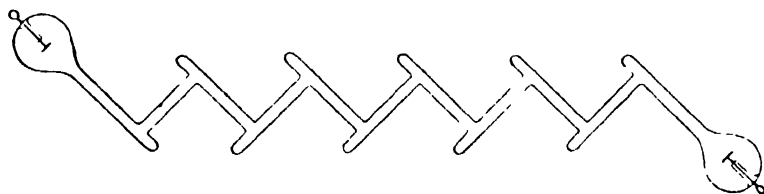


FIG. 2.

screen outside, or of taking a photograph through a sheet of aluminium. Even the brilliant tube which showed so excellently the shadow of the cross, fails to show any result after hours of vain waiting. It yields no rays that will penetrate aluminium. For experiments with Roentgen rays it is absolutely necessary that the process of exhaustion should be carried beyond the stage that suffices for the production of kathode shadows; it must be pushed to about that limit which Crookes himself described as his unit for the degree of vacuum, namely, one-millionth of an atmosphere. I do not say that with long exposures photographs cannot be taken when the degree of exhaustion is lower. Something depends, too, upon the degree to which the electric discharge is stimulated, and something also depends upon the shape and structure of the tube and upon the size and shape of the kathode. But on none of these things does the emission of X-rays depend so much as upon the degree of vacuum. The highly exhausted vacuum is the one real essential.

So soon as Crookes's researches upon electric shadows had become known, electricians set to work to try to produce electric shadows in ordinary air without any vacuum. One of the ablest of experimenters,

Professor W. Holtz, was successful, using as a source of electric discharge the electrified wind which is given off by a metal point attached to the pole of an influence machine. If in a perfectly dark room such a point is placed opposite and at a few inches from a wooden disc covered with white silk and connected at its back or edges to the other pole of the machine, it will be observed to show a pale luminosity over a circular patch where it is struck by the electric wind. If then the object is brought between the disc and the point a shadow will be observed to be cast upon the white surface. Non-conductors do not cast shadows as well as conductors do. A piece of thin mica scarcely casts a shadow at all until it is moistened. Double shadows can be got by using two disks covered with silk facing one another: any conducting object introduced between them casts a shadow on

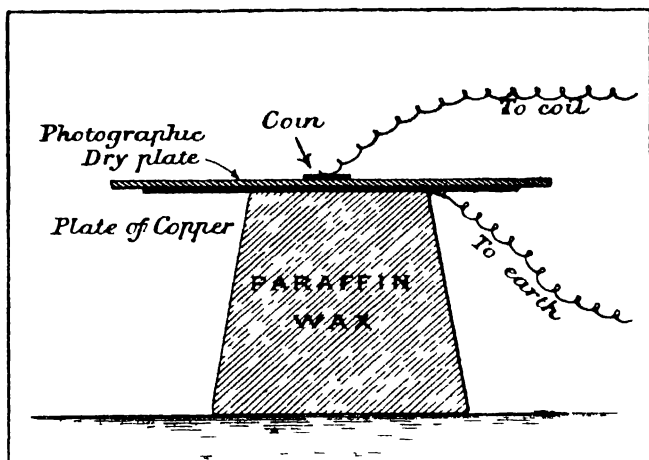


FIG. 3.

both. If such a shadow from an electrified point is cast downward upon a sheet of ebonite or pitch, the parts not shaded are found afterwards to remain electrified, and can be discovered by scattering over them Lichtenberg's mixed powders of red lead and lycopodium, thus perpetuating the shadow.

But now it is possible to produce electric shadows in another way, photographically, as has been known for some years,\* from metal objects such as coins, by simply laying them down upon a photographic dry-plate (a gelatino-bromide plate) and sending an electric spark (from an induction coil) into them.

Fig. 3 shows the arrangement adopted by the Rev. F. J. Smith, who is kind enough to exhibit in the library to-night some scores of his

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\* 'Proceedings Physical Society of London,' vol xi p 358, 1892

beautiful "inductoscript" photographs. Upon the screen I throw a few samples, including a print of one of the jubilee coins (Fig. 4). These curious photographs are produced simply by the chemical action of the electric discharges which stream off from all the projecting portions, and so roughly reproduce an image of the coin. Since Roentgen's discovery many persons have announced their supposed discovery of the production of electric shadow-pictures without the aid of a Crookes tube. What they have really observed is, however, totally different. They have not been producing X-rays at all, but have merely rediscovered these inductoscript shadows.

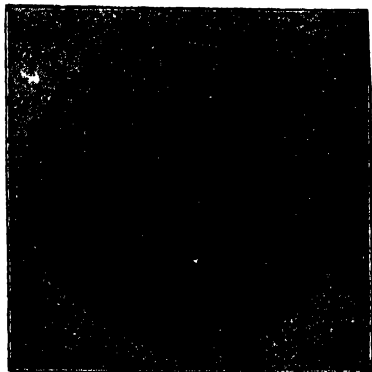


FIG. 4.

Between the researches of Crookes, however, and those of Roentgen there came in a very remarkable body of researches in Germany. I have but to name Goldstein,\* Puluj,† Hertz,‡

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\* Goldstein, in his researches on the Reflection of Electric (i.e. Kathode) Rays in 'Wiedemann's Annalen,' xv. 246, 1882, came very near to the discovery of the Roentgen rays. After pointing out that Hittorf had held the opinion that the kathode rays end at the place where they strike upon a solid wall, and that they are unable to proceed in any direction at all from thence, Goldstein directs attention to the circumstance that fluorescent patches are sometimes seen at the end of crooked tubes, where they could not have been caused by the direct impact of kathode discharges. He discusses the question whether this is due to reflection or to a deflection caused by the spot where impact first took place having become electrified negatively, and therefore acting as a secondary kathode. The latter hypothesis is rendered untenable by his observation that if the spot of first impact is made an anode the effect still occurs. He then shows that the phenomena are inconsistent with a specular reflection, but are explained by supposing that there is a diffuse reflection. He then sums up as follows:—"A bundle of kathode rays does not end, at least under those circumstances under which it excites phosphorescence, at the place where it strikes upon a solid wall, but from the place of impact on the wall there proceed electric rays in every direction in the gaseous space. These rays may be considered as reflected. Any solid wall of any property whatever may serve as a reflecting surface. It is immaterial whether or not it is capable of phosphorescence, or whether it consists of an insulator or of a conductor. The reflection is diffuse, no matter whether the surface is dull or most highly polished. An anode reflects the kathode rays sensibly as well as a neutral conductor or an insulator. The reflected rays have, like the direct kathode rays, the property to excite phosphorescence at their ends. They are subject to deflection, and their ends are deviated in the same sense as the ends of kathode rays, which would extend from the reflecting surface toward the place hit by the reflected rays."

† Puluj. "Radiant Electrode Matter and the so-called Fourth State." Published in vol. i. of 'Physical Memoirs,' by the Physical Society of London, 1889. These are translated from papers published in 1883 in the Memoirs of the Imperial Academy of Sciences of Vienna.

‡ H. Hertz. Researches on the Glow-Discharge, Wied. Ann. xix. 782, 1883. Hertz regards the kathode rays as a property of the ether, not as consisting of

Wiedemann,\* and Lenard,† amongst the workers, to show what interest has been concentrated on the subject. Hertz, whose loss science has not ceased to lament, observed that a part at least of the kathode rays were capable of passing through thin aluminium sheet, a property which confirmed him in his previous doubt as to the material nature of the cathodic discharge. His pupil, Philipp Lenard, now Professor Lenard, of Aachen, took up the point. He fitted up a tube with a small window of aluminium foil opposite the kathode,

moving particles. He finds the kathode rays to consist of a heterogeneous variety of kinds which differ from one another in their properties of causing phosphorescence, of being absorbed, and of being deflected by the magnet. On the Transmission of the Kathode Rays through Thin Layers of Metal, xlv. 28, 1892. Hertz finds that glass fluoresces in kathode rays, even if covered with gold leaf or thin films of various metals, though not if covered with thin mica. Aluminium was found best, and allowed fluorescence to occur even when a sheet of aluminium leaf was used so thick as to be opaque to light. A diaphragm of thin aluminium leaf on a metal frame placed inside a Crookes tube at 20 cm. from the kathode, permitted enough rays to pass to give a tolerably bright and even fluorescence over the whole of the further end of the tube. These rays, after passing through the leaf of metal, still showed rectilinear propagation (with some diffusion) and had not lost the property of being deflected by the magnet.

\* E. Wiedemann's papers which are of special importance have mostly appeared in 'Wiedemann's Annalen.' The following are the chief of them. Some of the later have been written in collaboration with Prof. H. Ebert.

On the Phosphorescent Light excited by Electric Discharges, Wied. Ann. ix. 157, 1880.

On Electric Discharges in Gases, xx. 756, 1881.

On Fluorescence and Phosphorescence, Pt. I. xxxiv. 446, 1888.

On the Mechanism of Luminosity, xxxvii. 177, 1889.

On Kathodo- and Photo-Luminescence of Glasses, xxxviii. 488, 1889.

On Electric Discharges in Gases and Flames, xxxv. 209, 220, 234, 237, 255, 1888.

On Electric Discharges, xxxvi. 643, 1889.

On the Apparent Repulsion of Parallel Kathode Rays, xvi. 158, 1892.

On Electric Discharges; Excitation of Electric Oscillations and the Relation of Discharge-tubes to the same, xlviii. 519, and xlix. 1, 1893.

Researches on Electrodynamic Screening-Action and Electric Shadows, xlix. 32, 1893.

Luminous Phenomena in Electrode-less rarefied Spaces under the Influence of rapidly alternating Electric Fields, l. 1, 221, 1893.

With J. B. Messerschmitt, on Fluorescence and Phosphorescence, Pt. II. Validity of Talbot's Law, xxxiv. 463, 1888.

With H. Ebert, on the Transparency of Kathode Deposits, Sitzber. d. phys.-med. Soc. zu Erlangen, Dec. 14, 1891.

† Lenard's papers are:—

Note on a Phosphoroscope, with spark illumination, Wied. Ann. xxxiv. 918, 1888.

With M. Wolf, Luminescence of Pyrogallic Acid, xxxiv. 918, 1888.

With V. Klatt, on the Phosphorescence of Copper, Bismuth, and Manganese in the Sulphides of Alkaline Earths, xxxviii. 90, 1889.

On Kathode Rays in Gases at Atmospheric Pressure, and in the most extreme vacuum, li. 225, 1894.

On the Magnetic Deflexion of the Kathode Rays, lii. 22, 1894.

On the Absorption of the Kathode Rays, lvi. 255, 1895.



its form being that shown in Fig. 5. The kathode was a flat disk on the end of a glass-covered wire stem. The anode was a cylindrical tube of brass surrounding the kathode. Upon the further end of the tube a brass cap was fixed by means of vacuum-tight cement. Over a small orifice in this brass cap was set the aluminium window of foil only  $\frac{1}{100}$  millimetre thick. By this means he was able to do what had previously been supposed impossible, bring the kathode rays out into the open air. Or, at least, that is what he appears to have considered that he was doing. Certainly he succeeded in bringing out from the vacuum tube rays that, if not actual prolongations of the kathode rays, were closely identified with them. He examined their properties both in the open air and in gases contained in a second chamber beyond the window, and found them to be capable of producing photographic impressions on sensitive plates. He further examined the question whether they can be deflected by a magnet. Fig. 6, which is copied from Lenard's paper, shows the results. The row of spots on the left side shows the photographic effect under various different conditions of experiment when there was no magnet present. The spots in the right-hand row show the effects obtained when a magnet was present. For example, in the third row from the top it is seen that the bundle of rays when subjected to the influence of the magnet is partially dispersed, the spot being enlarged sideways and having a kind of nebulous tail. This proves that through the aluminium window there came

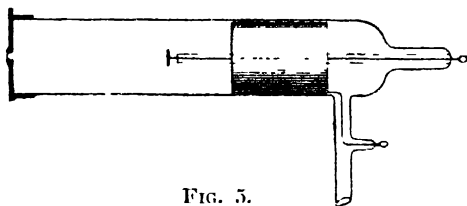


FIG. 5.

some rays which were deflected by a magnet, and some rays also which were not deflected by a magnet. The question naturally arises whether the rays which Lenard had thus succeeded in bringing out into the open air are the same thing as the rays with which Crookes

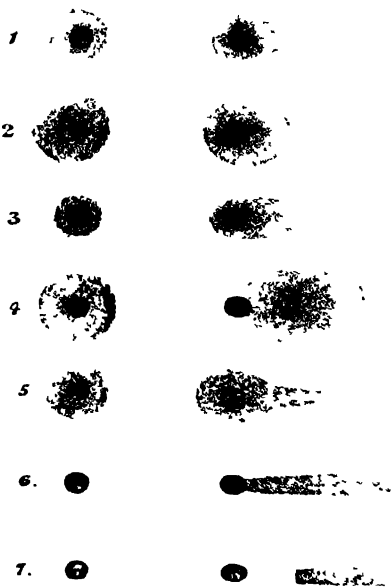


FIG. 6.

had been working with inside the vacuum. To that question the final answer cannot yet be given. Certainly some of the Lenard rays resemble the interior kathode rays: but some differ in the crucial respect of deflectability by the magnet. The higher the degree of vacuum, the less are the rays deflected.

Having touched all too briefly upon the researches of Lenard, it remains for me to speak of those of Wiedemann, of Erlangen, who for many years has made a study both of the phenomena of electric discharge and of those of fluorescence and phosphorescence. In a research made in the year 1895 he attained some results of singular interest. He had been making electric discharges, in collaboration with Professor Ebert, by a special apparatus for producing electric

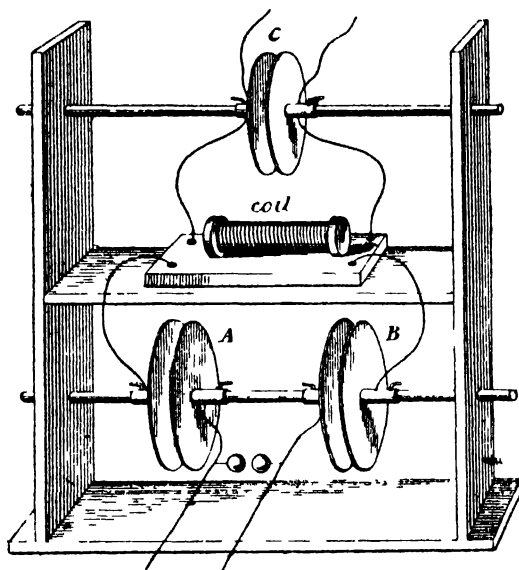


FIG. 7.

oscillations of high frequency. This apparatus, in the modified form given to it by Ebert,\* stands on the table before you. It is an apparatus of the same class as that described here some years ago by Oliver Lodge, for producing Hertzian waves. An oscillating spark is produced between two polished balls set between two condensers A and B, each made of plates of sheet zinc (Fig. 7) a few millimetres apart. Their external circuit is, however, led into the primary of a small induction coil, the secondary of which goes to a third condenser C. When sparks from the Apperly coil are sent to the spark-gap, the oscillations of the two primary condensers set up secondary oscilla-

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\* Wiedemann's *Annalen*, liii. p. 144, 1894.

tions in the third condenser, to which a vacuum tube can be connected. If, now, by adjusting the distances between the plates of condensers we tune the primary and secondary circuits together, the electric oscillations that result will persist much longer than if the circuits are not so tuned. Though each oscillation may last less than the 100-millionth of a second, there will be at each spark some 20,000 or 30,000 oscillations before they have died out. Wiedemann and Ebert have found that these persistent oscillations are specially adapted to excite luminescence. To illustrate the point I select here an old Geissler tube with a comparatively poor vacuum. When stimulated by ordinary sparks directly from the Apps coil through the platinum electrodes at its ends, it shows the usual features of Geissler tubes: there is a luminous column extending through the central bulb with stratifications along its length, while around the kathode is the usual violet glow. The glass shows no fluorescence. I now change the connections, uniting the wires from Ebert's apparatus, not to the terminal electrodes of the tube but to two patches of tin-foil stuck upon the outside of the central bulb. Under these conditions the electric oscillations illuminate the central bulb with a glow quite different from that previously seen. Beneath each patch of foil you can discern the bluish kathode discharge, and the glass now shines with characteristic apple-green fluorescence. By moving one plate of one of the condensers in or out I alter the conditions of resonance in the circuit; and when the tuning is best the fluorescence is at its brightest. Now Wiedemann observed\* that the light so generated is capable of exercising a photographic action and of other effects, but is incapable either of passing through a thin plate of fluor-spar or of being deflected by a magnet. These rays differed therefore both from ultra-violet light and from kathode rays; hence Wiedemann pronounced them to consist of a new species which he named "Entladungsstrahlen" or discharge-rays. It is again a matter for research to determine whether Wiedemann's rays are the same as Lenard's, or as Roentgen's rays. Wiedemann's coadjutor Ebert went on with the research and produced on this principle a little "luminescence lamp" having two external rings of foil as electrodes; and within the vacuum bulb a small pastile of phosphorescent stuff, which, when excited by the oscillations of the tuned circuits, glows with a small bright light. Ebert claims that its efficiency is many times greater than that of the ordinary glow lamp.

Returning now to Roentgen's researches, we will take a glance at the kind of tube (Fig. 8) which he was employing when he made his discovery of the X-rays. Its general resemblance to previous tubes† is self-evident. The anode was a piece of aluminium tube through which passed the glass-covered kathode wire, with a small

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\* 'Zeitschrift für Elektrochemie,' July 1895, p. 159.

† It is, in fact, identical with the form described by Hertz in 1883, see 'Wiedemann's Annalen,' xix. p. 810.

flat aluminium plate on its extremity. From this flat plate the kathode rays shot forward against the bulging end of the tube; and, without any aluminium window rays which were capable of exciting

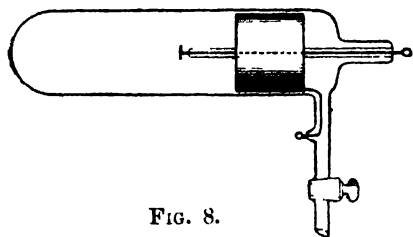


FIG. 8.

fluorescence, found their way through the glass walls. Lenard had so boxed up his tube with brass cap and metal case, that if anything in the way of rays struggled through the glass walls of his tube he might not notice it. Possibly he never looked for it. Roentgen made the fortunate observation that when his tube was

closely covered with opaque black card it still could cause fluorescence on a screen covered with platino-cyanide of barium on which shadows were cast. From seeing the shadows thus to securing their imprint permanently on a photographic plate was but a small step, and the discovery that they could pass freely through a sheet of the metal aluminium was a natural result of an inquiry as to the transparency of different materials. Aluminium is to these rays much more transparent than ordinary glass. No lens can focus them, nor mirror reflect them; and, unlike the kathode rays within the tube, they are not deflected by the magnet.

The criterion which we have at present as to whether any rays from any other source are or are not the same as the X-rays is that they shall be able to fulfil the following four-fold test:—They must be capable of exciting luminescence; they must be capable of impressing an image on a photographic plate; they must be capable of passing through aluminium; and they must be incapable of being deflected by a magnet. In addition they must—so far as present evidence goes—be incapable of being either refracted or polarised. Any rays that will fulfil these tests must for the present be considered identical with X-rays.

Now it has been suggested that the X-rays are the same as ultra-violet light. This is certainly not so, for ultra-violet light, as known to us by the researches of Stokes, Tyndall, Becquerel and Cornu, will not go through aluminium and is not deflected by a magnet, though it will excite luminescence and take photographs. Furthermore ultra-violet light can be refracted and polarised.

It has also been suggested that the X-rays are merely invisible heat-rays. But this is certainly untrue also, because although Abney has succeeded in taking photographs by heat rays, they will not go through aluminium, are not deflected by the magnet, and instead of exciting phosphorescence they destroy it, as Goethe found out nearly a hundred years ago.

Neither are they Hertzian waves of longer period than the heat waves.

So far as is at present known there is no other way of producing

the X-rays than that of employing the highly exhausted vacuum tube. They are not found in the light of ordinary electric sparks in air. They are not discoverable amongst the rays emitted by ordinary Geissler tubes with a low exhaustion. They are not found in sunlight or any artificial light. They are light, though it yields rays that will give photographic shadows through a thin pine-wood board, yields no rays that will pass through aluminium. The only other rays that seem to come within reasonable possibility of being X-rays are the Lenard rays, some of which are probably identical with Roentgen's; the Wiedemann rays, which are, so far as yet investigated, entirely similar; and the Becquerel rays, to which some allusion will presently be made. It will, however, be convenient here to present a synoptic table (see p. 502) of the various kinds of rays and their respective physical properties.

One other physical property of the X-rays has been discovered since the publication of Roentgen's research. It was discovered simultaneously in Cambridge (by Professor J. J. Thomson), in Paris, in Bologna, and in St. Petersburg, that these X-rays will cause the diselectrification of an electrified body, no matter whether it is positively or negatively charged.\* That ultra-violet light can diselectrify bodies that have been negatively charged was previously known from the researches of Hertz, and of Elster and Geitel. This fresh discovery that X-rays will also discharge a positive electrification sets up a new physical test. Let me show you a simple piece of apparatus which I have found very convenient for the purpose of demonstrating this discovery. It is an aluminium-leaf electroscope (Fig. 9) entirely shielded from all external electrostatic influences by being enclosed in transparent metallic gauze. It is so well shielded that even when the cap is removed it cannot be charged in the ordinary inductive way, but must be electrified by direct conduction. The aluminium leaves hang at the side of a fixed central plate as in Exner's electroscope. The containing vessel is of thin Bohemian glass. On exciting the instrument positively from a rod of rubbed glass, or negatively from a rod of rubbed celluloid, the leaves diverge. In either case as soon as the X-rays are caused to shine upon the instrument the leaves fall.



FIG. 9.

It occurred to me that by the aid of this property of diselectrification it might be possible to produce electric shadows without having resort to any photography. You are aware that if the surface or any part of the surface of a body is electrified, the fact that it

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\* It is of great interest to note that this identical property had been observed by Lenard a year previously as an effect of his rays. He found they would discharge an electroscope enclosed in a metal chamber, with an aluminium sheet in front, whether positively or negatively charged, and at a distance of 30 centimetres from his tube.

TABLE I.

Kind of Rays	Cause Luminescence	Penetrate Aluminium	Cause Photographic Action	Cause Combination of H and Cl	Deflected by Magnet	Discharge Electrification	Affect Spark Length	Change Nature of Electric Discharge	Restore Thermoluminescence	Capable of being Polarised	Capable of Refraction, &c.
Ultra-violet light	Yes	..	Yes	Yes	No	If —	Yes	..	Yes	Yes	Yes
Infra-red light	No	No	..	No	No	No	..	..	No	Yes	Yes
Hertzian waves	No	No	..	No	No	..	Yes	..	No	Yes	Yes
Kathode rays	Yes	If thin	Yes	..	Yes	{ charge } { violently }	..	..	..	..	..
Lenard rays	Yes	Yes	Yes	..	Partly	Yes	Yes	Yes	..	..	..
Wiedemann rays	Yes	Yes	Yes	..	No	..	..	..	Yes	..	..
Roentgen rays	Yes	Yes	Yes	..	No	Yes	Yes	Yes	Yes	No	No
Bequerel rays	..	Yes	Yes	..	No	Yes	..	..	..	Yes	..
Electric effluve	Yes	No	Yes	Yes	?	..	..	..	Yes	No	No

is electrified can be ascertained by dusting over it mixed powders of red lead and sulphur (or red lead and lycopodium). With the aid of Mr. Miles Walker, who has worked with me all through this matter, I have succeeded in producing, on this plan, well-defined shadows which will now be demonstrated to you. A clean sheet of ebonite freed from all traces of previous electrification by being passed through a spirit flame is laid on a properly prepared metal table. On it stands a small tray of thin aluminium supported on four insulating legs. In this tray is placed the object whose shadow is to be cast, for example a pair of scissors or an object cut out in sheet lead. Over this again is placed a leaden cover with an opening above the tray: the leaden cover being designed to cut off electrostatic influences which might interfere. The tray is then electrified by a small influence machine, and while it is so electrified X-rays are sent downwards from a Crookes tube placed above. They pass down through the aluminium tray and carry its electrification to the ebonite sheet, which therefore becomes electrified all over except in the parts which are shielded by the scissors or other metallic object. The sheet of ebonite is then removed from the leaden enclosure, the aluminium tray lifted off, and the mixed powders are dusted over, adhering to the surface of the ebonite and revealing the otherwise invisible electric shadow. Fig. 10 is a shadow taken in this way. It is but right to mention that Professor Righi, of Bologna, has independently obtained electric dust shadows in a very similar way since these experiments of mine were begun.

This will be a convenient place to mention a new effect of X-rays which I have recently observed and which is set down in the table. When X-rays fall upon a metal object electrified by an influence machine, they produce some curious changes in the nature of the discharge into the air. If the body is already discharging itself from some edge or corner in an *aigrette* or brush discharge (visible in darkness only) the size and form of the aigrette is much altered. Under some circumstances not yet investigated, the incidence of X-rays causes the aigrette to disappear; under others the X-rays provoke its appearance.

Since the publication of Roentgen's research the most notable advance that has been made has been in the direction of improving the tubes. Roentgen himself has mostly employed a pear-shaped tube with a flat circular kathode near the top, producing a beautiful fluorescence of the lower part of the tube. He carefully verified the circumstance that the X-rays originate at that portion of the glass surface which receives the impact of the cathodic discharge. They appear in fact to be generated at the place where the kathode discharge first impinges upon the surface of any solid body. It is not necessary that the substance which is to act as emitter of the X-rays should become fluorescent. On the contrary, it appears that the best radiators are substances that do not fluoresce, namely the

metals. I have found zinc, magnesium, aluminium, copper, iron and platinum to answer—the last two best.\* Mr. Porter, of University College, and Mr. Jackson, of King's College, have independently found out the merits of platinum foil, the former using an old Crookes tube designed for showing the heating effect of the kathode discharge when

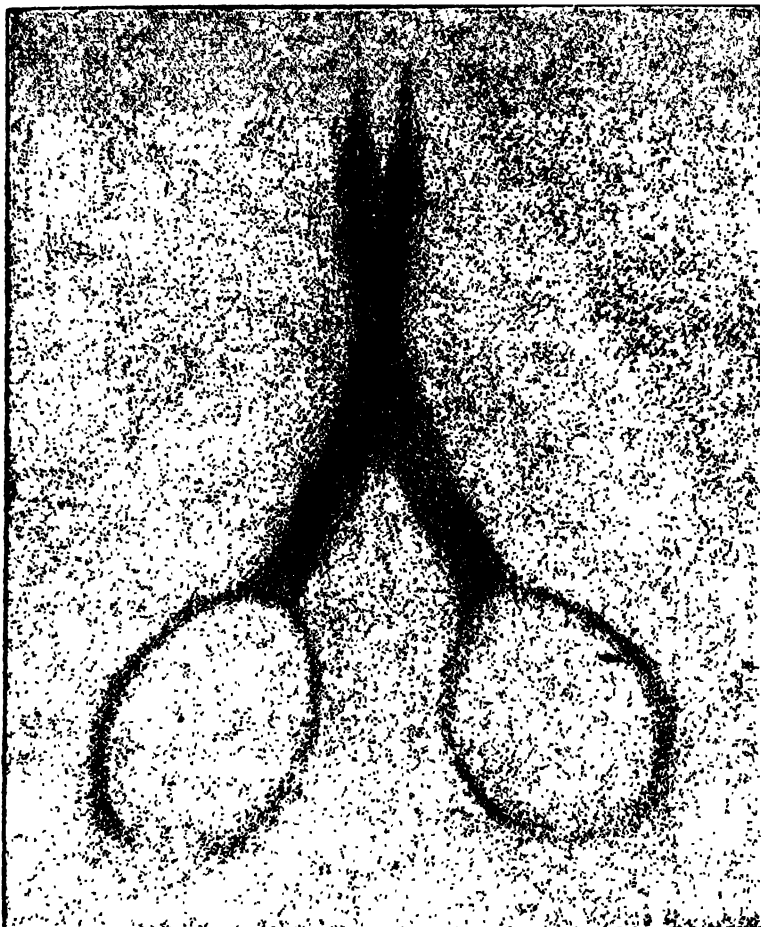


FIG. 10.

concentrated by a concave kathode. On the table are some of the experimental forms † of tubes I have used. The best results are found when the kathodic discharge is directed against an interior

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\* [The author has since found metallic uranium to surpass all other metals.]

† See 'Philosophical Magazine,' August 1896, p. 162.



piece of metal—preferably platinum—which I term the antikathode \* set obliquely opposite the kathode, and which serves as a radiating surface from which the X-rays are emitted in all directions. When experimenting with various forms of tube, I have spent much time in watching, by aid of a fluorescent screen, their emissive activity during the progress of exhaustion. As already mentioned, X-rays are not emitted until the stage of minimum internal resistance has been passed. As the exhaustion advances, while resistance rises and spark length increases, there is noticed by aid of the screen a luminosity in the bulb, which, faint at first, seems to come both from the front face of the bit of platinum that serves as antikathode, and from the back face ; an oblique dark line (Fig. 11), corresponding to the plane of

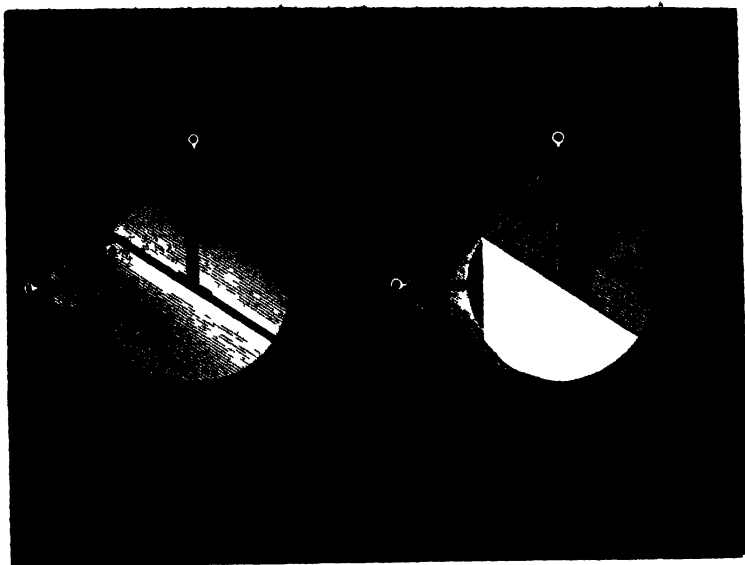


FIG. 11.

FIG. 12

the antikathode, being observed in the screen to separate the two luminous regions. On slightly increasing the exhaustion the emission of X-rays from the back of the antikathode ceases while that from the front greatly increases (Fig. 12), and is quite bright right up to the angle delimited by the plane of the antikathode. There is something mysterious, needing careful investigation, in this lateral emission of X-rays under the impact of the kathode discharge.

Of all the many forms of tube yet produced none has been found to surpass the particular pattern devised by Mr. Sydney Jackson.

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\* Comptes Rendus, cxxx. p. 507

(Fig. 13), and known as the "focus tube." It was with such a tube that I showed you at the outset the fundamental experiments of Roentgen. A concave polished kathode of aluminium concentrates the cathodic discharge upon a small oblique sheet of platinum, which, while acting as antikathode, serves at the same time as anode. Not only does the concentration of the cathodic discharge upon the metal cause it to emit X-rays much more vigorously, but it also has the effect of causing them to be emitted from a comparatively small and definite source, with the result that the shadows cast by opaque objects are darker. [Photographs were then thrown upon the screen, those taken with "focus" tubes showing remarkable definition of detail. Some of these were by Mr. J. W. Giffen; others, showing diseased bones, &c., taken by the lecturer, and some by Mr. Campbell-Swinton and by Mr. Sydney Rowland, were also projected.]

The objection has been taken that in these shadow photographs it is impossible to distinguish the parts that are behind from those that

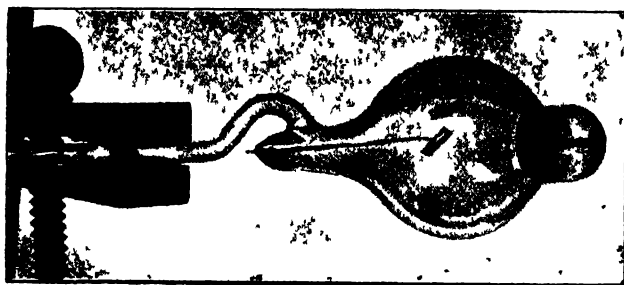


FIG. 13.

are in front. In a sense that is so. But I venture to say that the objection not only can be got over but has been got over. I cannot show the proof of my assertion upon the screen, because I cannot put upon the screen a stereoscopic view. But here in my hand is the Roentgen stereograph of a dead tame rabbit. Two views were taken, in which the X-rays were thrown in two different directions at an angle to one another. When these two views are stereoscopically combined, you observe the rabbit's body with the lungs and liver inside in their relative positions. The soft organs, which cast faint shadows almost indistinguishable amid the detail of ribs and other tissues, now detach themselves into different planes and are recognisable distinctly. I now send up for projection in the lantern the two photographs that were taken at the beginning of my discourse, and which have in the meantime been developed.

Turning back to the phenomena of luminescence,\* permit me to

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\* This very convenient term was suggested some six years ago by Wiedemann, to denote the many phenomena known variously as fluorescence or

draw your attention to the accompanying table of the different kinds of luminescence with which the physicist has to deal.

TABLE II.

<i>Phenomenon.</i>	<i>Substance in which it occurs.</i>
1. Chemi-luminescence .. ..	Phosphorus oxidising in moist air; decaying wood; decaying fish; glow-worm; fire-fly; marine organisms, &c.
2. Photo-luminescence :	
(a) <i>transient</i> = Fluorescence ..	Fluor-spar; uranium-glass; quinine; scheelite; platino-cyanides of various bases; eosin and many coal-tar products.
(b) <i>persistent</i> = Phosphorescence	Bologna-stone; Canton's phosphorus and other sulphides of alkaline earths; some diamonds, &c.
3. Thermo-luminescence .. ..	Scheelite; fluor-spar.
4. Tribo-luminescence .. ..	Diamonds; sugar; uranyl nitrate; pentadecylparatolylketone.
5. Electro-luminescence :	
(a) Effluvio-luminescence .. ..	Many rarefied gases; many of the fluorescent and phosphorescent bodies.
(b) Kathodo-luminescence .. ..	Rubies, glass, diamonds, many gems and minerals.
6. Crystallo-luminescence .. ..	Arsenious acid.
7. Lyo-luminescence .. ..	Sub-chlorides of alkali-metals.
8. X-luminescence .. ..	Platinocyanides, scheelite, &c.

You will note the names given to discriminate from one another the various sorts of luminescence. Chemi-luminescence denotes that due to chemical action, as when phosphorus oxidises, or when the glow worm emits its cold light. Then there is the photo-luminescence of the bodies which shine when they are shone upon. There is the thermo-luminescence of the bodies which shine when heated. There is tribo-luminescence caused by certain substances when they are rubbed. There is the kathodo-luminescence of the objects placed

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phosphorescence. It refers to all those cases in which light is produced, whether under the stimulus of electric discharge, of heat, of prior exposure to illumination, or of chemical action, and the like, in which the light is emitted at a lower temperature than that which would be necessary if it were to be emitted by means of incandescence.

in a Crookes tube. There is the crystallo-luminescence of certain materials when they become solid; and the lyo-luminescence of certain other materials when they are dissolved. Lastly, there is the X-luminescence set up by the X-rays.

Pausing on photo-luminescence, here is an experiment to illustrate the difference between its two varieties, phosphorescence and fluorescence. Light from an arc lamp, filtered from all rays except violet and ultra-violet, is thrown upon a disk to which rapid rotation is given by an electric motor. The disk is painted with two rings, one of sulphide of calcium, the other of tungstate of calcium. Though the light falls only on one patch you note that the sulphide shows a continuous ring of blue light, for the emission of light persists after the stuff has passed out of the illuminating rays. The tungstate, on the other hand, shows only a short trail of light, the rest of the ring being non-luminous, since tungstate has but little persistency. The light has in fact died out before the stuff has passed a quarter of an inch from the illuminating beam. This is a sort of phosphoroscope designed to show how long different materials will emit light after they have been shone upon. Those which show only a temporary luminescence are called fluorescent, while those with persistent luminescence are called phosphorescent. For many years it has been known that some diamonds are phosphorescent. Three such are here shown,\* which, after exposure of one minute to the arc light, shine in the dark like glow-worms. The most highly phosphorescent material yet produced is an artificial preparation of sulphide of calcium manufactured by Mr. Horne. The specimen exhibited has a candle-power of about  $\frac{1}{10}$  candle per square inch after exposure for a few seconds to direct sunlight; but the brilliancy rapidly dies away, though there is a visible luminescence for many days. This substance is also brightly luminescent in a Crookes tube, and less brightly under the influence of X-rays.

Many substances, notably fluor-spar, have the property of thermo-luminescence, that is they shine in the dark when warmed. Powdered fluor-spar dropped upon a hot shovel emits bright light. If, however, the spar is heated to a temperature considerably below red heat for some hours, it apparently comes to an end of its store of luminous energy and ceases to shine. Such a specimen, even after being kept for some months, refuses to shine a second time when again heated. It has, however, long been known that the property of luminescing when warmed can be restored to the spar by passing a few electric sparks over it, or by exposing it to the silent discharge or aigrette. Wiedemann having found that the cathode rays produce a similar effect, it occurred to me to try to find out whether any of these X-rays also would revivify thermo-luminescence. I have found that on exposure for twenty minutes to X-rays, a sample of fluor-spar

which had lost its thermo-luminescent property by prolonged heating was partially though not completely revived.

I referred earlier to the rays recently discovered by M. H. Becquerel. In February last M. Becquerel, and independently I myself,\* made the observation that uranium salts emit some rays which very closely resemble the X-rays, since they will pass through aluminium and produce photographic action. It remains to be seen whether these rays are identical with those of Roentgen.

Finally, let me briefly exhibit two results of my own work. There is now shown (Fig. 14) the photographic shadow of two half-hoop ruby rings. One of them is of real rubies, the other of imitation stones. By artificial light it is difficult to distinguish one from the other, but when viewed by the X-rays there is no mistaking the false for the true. The real rubies are highly transparent, those of glass are practically opaque.

After gaining much experience in judging by photography of the relative transparency of materials, I made a careful research† to discover whether these rays can be polarised. At first I used tourmalines of various thicknesses and colours. More recently I have tried a number of other dichroic substances, andalusite, sulphate of nickel, of nickel and ammonium, sulphate of cobalt, and the like. The method used for all was the following. A slice of the crystal was broken into three parts. One part was laid down, and upon it were superposed the other two in such a way that in one the crystallographic axis was parallel, in the other perpendicular, to the crystallographic axis in the first piece. If there were any polarisation the double thickness where crossed in structure would be more opaque than the double thickness where the structure was parallel. Not the slightest trace of polarisation could I observe in any case. Of numerous other observers who have sought to find polarisation, none has yet produced a single uncontestable case of polarisation.

At the present moment interest centres around the use of luminescent screens for observing the Roentgen shadows, and in this direction some advances have been claimed of late. It should, however, not be forgotten that Roentgen's original discovery was made with a screen covered with platino-cyanide of barium. Here is a piece of card covered with patches of several different kinds of lumi-

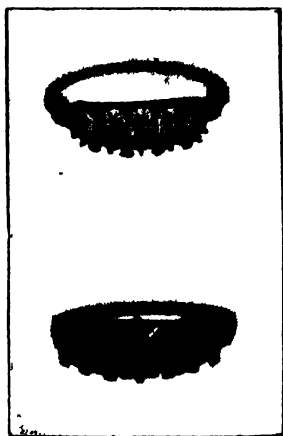


FIG. 14.

\* See 'Philosophical Magazine'; July 1896.

† *Ib.* August 1896.

nescent stuffs, several platino-cyanides, several sulphides, and some samples of tungstate of calcium. Of these materials the brightest in luminescence is the hydrated platino-cyanide of potassium employed by Mr. Sydney Jackson; the next brightest is a French sample of platino-cyanide of barium; platino-cyanide of strontium coming third.

Using a focus tube of Mr. Jackson's improved pattern, enclosed in a box with a cardboard front, and taking a platino-cyanide screen, I am able in conclusion to demonstrate to all those of my audience who are within a few feet of the apparatus, the facts that have so startled the world. You can see the bones of my hand and of my wrist. You can see light between the two bones of my forearm; while metal objects, keys, coins, scissors, &c., enclosed in boxes, embedded in wood blocks, or locked up in leather bags, are plainly visible to the eye.

Whatever these remarkable rays are, whether they are vortices in the ether, or longitudinal vibrations, or radiant matter that has penetrated the tube, or, lastly, whether they consist simply of ultra-violet light, their discovery affords us one more illustration of the fact that there is no finality in science. The universe around us is not only not empty, is not only not dark, but is, on the contrary, absolutely full and palpitating with light: though there be light which our eyes may never see, and sounds which our ears may never hear. But science has not yet pronounced its last word on the hearing of that which is inaudible and the seeing of that which is invisible.

[S. P. T.]

Friday, May 22, 1896.

GEORGE MATTHEY, Esq. F.R.S. Vice-President, in the Chair.

PROFESSOR J. A. EWING, M.A. F.R.S. Professor of Mechanism and Applied Mechanics in the University of Cambridge.

*Hysteresis.*

(Abstract.)

THE lecturer explained that the word hysteresis was not a term in neuro-pathology. It had nothing to do with hysterics. The name might be unfamiliar, but the thing it described was exceedingly common. It was scarcely too much to say that hysteresis was to be found everywhere, except, perhaps, in the dictionary.

The word was derived from the verb *ὕστερέω*, which signified to lag behind. It was introduced about fourteen years ago to name a characteristic which had been prominent in several researches into the physical qualities of certain materials, especially of iron. The name was invented at a time when the phenomenon of hysteresis had no more than a purely scientific interest; but in the rapid advance of industrial electricity hysteresis had become a matter of much commercial importance, and the word was now in common use by electrical engineers. Certain materials, when causes acted on them tending to change their physical state, had a tendency to persist in their previous state. This tendency to persist was what constituted hysteresis.

It was in connection with the magnetic properties of iron and steel that the most conspicuous and practically the most important manifestations of hysteresis were found. An experiment was shown to illustrate hysteresis in the changes of magnetic condition brought about by the application and removal of stress. An iron wire, magnetised by a constant current in a surrounding coil, was hung up and loaded with weights. The weights were alternately removed and reapplied, and the magnetic state of the wire was shown by means of a mirror magnetometer. It was seen that when the weights were repeatedly put on and off, the magnetism changed from one to another of two values; but when half the weight only was left on during unloading, the magnetism assumed a value much nearer to the loaded than to the unloaded state; whereas when half the weight was put on after unloading, the magnetism took a value nearer the unloaded than the loaded state. In other words, the magnetic effects of the loading lagged behind the changes in the loading itself. This

lagging was shown to be static in character, for it was in no way dependent on the rate at which the process of loading and unloading was performed. Other cases of static hysteresis in the thermoelectric and mechanical qualities of iron were mentioned.

Practically the most important instance was the hysteresis which was observed when a piece of iron had its magnetism changed by changing the magnetising force. When a piece of iron was first magnetised, the magnetism  $B$  was developed by gradual increase of the magnetising force  $H$ , in the way shown in Fig. 1. If at any stage in the process, such as  $a$ , the magnetising force was made to stop increasing, was reduced to zero, and was then reapplied in the opposite direction, the magnetism changed in the way shown by the curve  $acd$  of Fig. 2. And finally, if the magnetising force were again reversed, so as to recover the direction and value it had at  $a$ , the process followed was represented by the curve  $dea$  of Fig. 3.

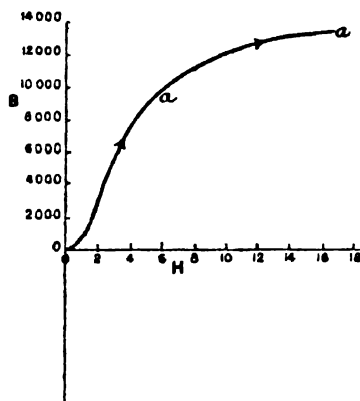


FIG. 1.

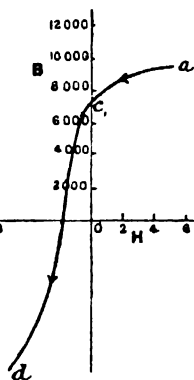


FIG. 2.

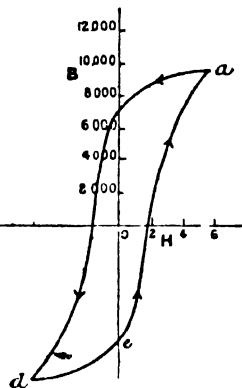


FIG. 3.

The closed curve of Fig. 3 showed how the changes of magnetism in this complete cycle of double reversal tended to lag behind the changes of magnetising force. In consequence of this hysteresis, energy was consumed in reversing the magnetism of iron, and it could be proved that the energy consumed in each double reversal was proportional to the area enclosed between the curves  $acd$  and  $dea$ .

This was the process that went on in the iron cores of transformers when used for electric lighting in the alternate-current system of supply. The magnetic cycle was gone through something like 100 times a second, and as a rule the transformer was in circuit continuously by day and night. Whether it was supplying lamps and doing useful service, or whether it was not, the waste of power due to hysteresis went on. It formed a very serious item in the cost of alternate-current supply, for the effect was that a large part of



the coal burnt at the central station, after having its energy passed through a series of costly conversions, was devoted in the end to nothing more than uselessly warming the transformers in the cellars of consumers or in boxes under the streets. So long as iron could not be found that was destitute of magnetic hysteresis, some loss on this account was inevitable; but it might be greatly lessened by choosing a suitable kind of iron. Experience showed that some kinds of iron had much less hysteresis than others. Thus in Fig. 4 the curve marked I related to a specimen of iron eminently suitable for use in transformers, while the curves marked II and III related to other brands of iron. They enclosed much larger areas, and showed

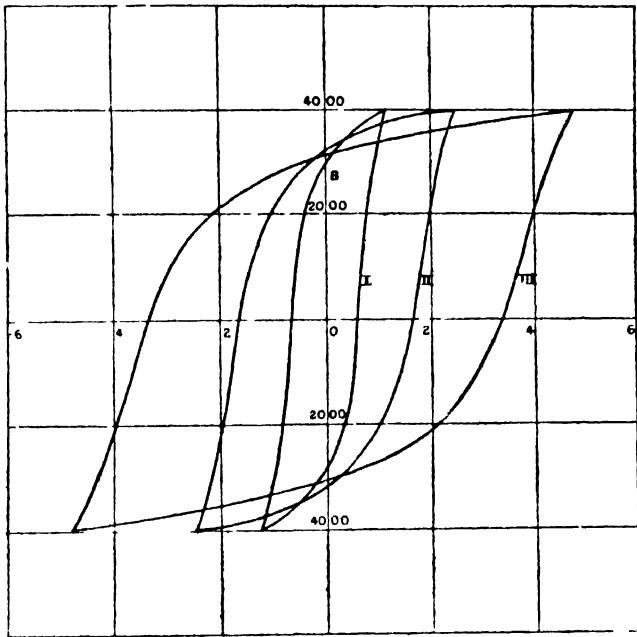


FIG. 4.

that the iron which gave them was to be avoided as having too much hysteresis. Of late years some of the makers of iron had striven with marked success to produce iron which should be comparatively free from hysteresis, and it was now possible to obtain material for transformers which reduced the loss to a fraction of what was formerly thought inevitable.

The lecturer's magnetic curve tracer was exhibited in action, showing magnetic curves, similar to Fig. 3, upon a screen by giving to a small mirror simultaneous horizontal and vertical movements, the former proportional to the magnetising force, and the latter to the magnetisation of the specimen of iron in the machine. As a

convenient means of practically testing the quality of iron in this respect the lecturer had lately introduced another instrument, which was also shown at work. In this hysteresis tester (Fig. 5) the sample of iron, in the form of a bundle of thin strips, was clamped in a carrier and caused to rotate between the poles of a magnet swinging on knife edges. As a consequence of hysteresis this magnet was deflected, and its deflection, which was noted by means of a pointer and scale, served to measure the hysteresis.

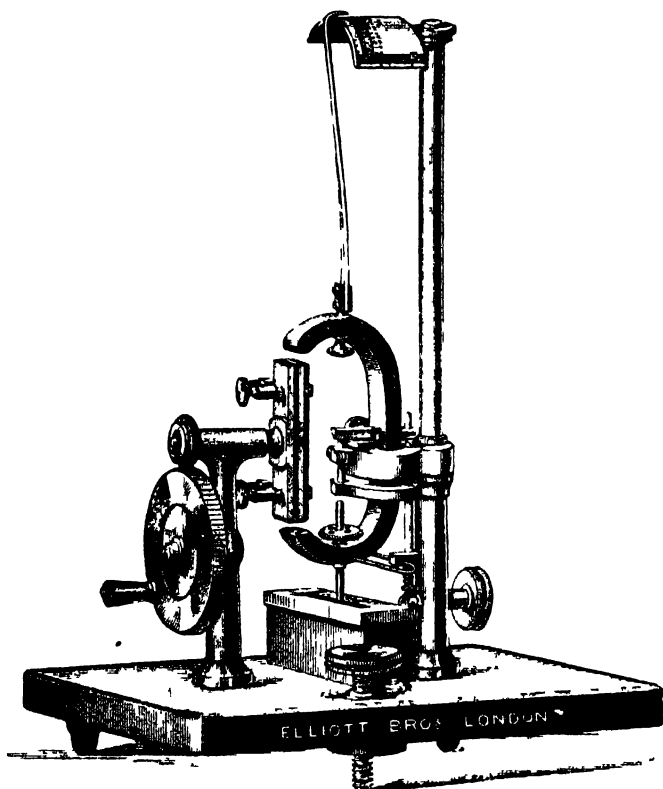


FIG. 5

To show directly the heating effect of magnetic reversals in iron, a differential air thermometer was used, with long bulbs, one of which was partly filled by a bundle of iron wire. Both bulbs were surrounded by coils, through which an alternating current passed. The heating effect of the current itself was the same for both, but the bulb containing the iron was further heated in consequence of the hysteresis of the metal, and this additional heating was shown by movement of a liquid index in a tube connecting the two bulbs. It had even been proposed to apply the heating effect of hysteresis

to the boiling of water. A kettle invented for this purpose by Sir David Salomons and Mr. Pyke was exhibited.

In another experiment to illustrate the dissipation of energy through magnetic hysteresis a steel ball was caused to roll down an inclined railway formed by a slot cut in an iron tube. The tube was wound longitudinally with a magnetising coil which caused lines of magnetic induction to cross the slot. The ball was consequently magnetised, and as it rolled the changes of magnetism in it and in the neighbouring parts of the tube checked its motion, causing it to slow down or stop when the current in the magnetising wire was applied; but the resistance due to hysteresis ceased when the current was broken.

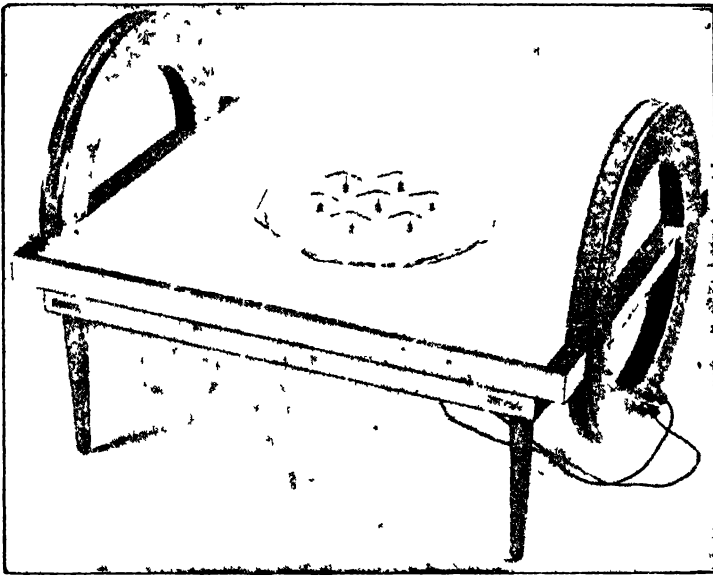


FIG. 6

In conclusion the lecturer referred to the molecular theory of magnetisation, which he had explained in a former lecture,\* and to the explanation it gave of magnetic hysteresis. Since then it had received a remarkable confirmation from the work of Mr. F. G. Baily, who had measured the hysteresis when iron discs were made to revolve in a strong magnetic field. He found that when the field was strengthened the hysteresis was at first increased, but a stage was reached when the strengthening of the field ceased to increase the hysteresis, and with a stronger field still the hysteresis was

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\* 'Proceedings,' Royal Institution, May 22, 1891.

actually reduced. Indeed, by a small further increase of the field the hysteresis could be made to practically vanish. This very curious result had been predicted originally by Mr. James Swinburne, as a consequence of the lecturer's theory, and had at that time seemed so unlikely that it was urged as an objection to the theory. It had now been proved to afford the theory the strongest possible confirmation.

A model was shown in illustration of this point, in which a glass plate carrying a number of small pivoted magnets (Fig. 6) was made to revolve slowly in a magnetic field produced by two neighbouring coils. So long as the field was weak the small magnets formed groups which were broken up during the revolution, thereby dissipating energy and exhibiting hysteresis; but when the field was sufficiently strengthened the small magnets continued to point one way without forming groups, for their mutual magnetic forces were then masked by the external or field force. There were consequently then no unstable phases in the motion and no hysteresis.

Hysteresis in the magnetic quality of iron was to be ascribed to the formation of stable groups of molecules, in consequence of the mutual forces which the molecules exerted on one another in virtue of their magnetic polarity. It might very well be that in other manifestations of hysteresis, such, for example, as the familiar phenomenon of friction between two solid surfaces when rubbing against one another, the resistance and consequent dissipation of energy were similarly due to the forming and breaking up of molecular groups, the molecules being mutually constrained by some other species of polar forces, possibly due to electrostatic charges upon them.

[J. A. E.]

Friday, June 5, 1896.

THE RIGHT HON. LORD KELVIN, D.C.L. LL.D. F.R.S. Vice-President,  
in the Chair.

PROFESSOR J. A. FLEMING, M.A. D.Sc. F.R.S. *M.R.I.*

*Electric and Magnetic Research at Low Temperatures.*

DURING the last four years much time has been spent by Professor Dewar and by me in the prosecution of a joint research on the principal electric and magnetic properties of metals at very low temperatures. Some reference has already been made in previous discourses by Professor Dewar to portions of this work,\* but the special object of the present lecture is to extend these descriptions, and put you in possession of the latest results in this department of the low temperature investigations. It will be convenient to discuss the several divisions of it in the order in which they have engaged our attention.

One hundred and sixty-seven years ago Stephen Gray, a pensioner of the Charterhouse, in conjunction with his friend Granville Wheler, stretched a packthread 300 feet long over silk supports, and demonstrated that an electrification of the thread at one end spread instantly over the whole mass, but that if metal wires replaced the silk no electrification of the thread was possible. This experiment undoubtedly formed the starting-point for the first definite recognition of the necessity for a classification of bodies into insulators and conductors, a distinction which Gray's brilliant contemporary, Dufay, extended and confirmed, and for which he and Desaguliers coined these familiar terms.† Gray's contributions to knowledge as an epoch-making discoverer have received less notice from scientific historians than their real value deserves. It is less easy to state who first noticed that the powers of conduction and insulation were greatly affected by temperature. Cavendish, in 1776, however, was perfectly familiar with the fact that solutions of common salt conduct electricity better when warm than when cold,‡ and made measurements of the relative

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\* 'Scientific Uses of Liquid Air.' A Friday Evening Discourse, by Professor J. Dewar, LL.D. F.R.S. delivered at the Royal Institution, Jan 19, 1894.

† See the 'Intellectual Rise in Electricity,' by Park Benjamin. Stephen Gray's papers on this subject, communicated to the Royal Society, are as follows: Phil. Trans. 1720, vol. xxxi. p. 104; 1731, vol. xxxvii. p. 18; 1732, vol. xxxvii. p. 285; 1735, vol. xxxix. p. 16; 1736, vol. xxxix. p. 400. See also Dufay, Phil. Trans. 1733, No. 431, p. 258.

‡ See the 'Electrical Researches of Cavendish.' Edited by Clerk-Maxwell, p. 324.

resistances of an iron wire and a salt solution which were marvellously accurate, when we consider that his only means of measurement was the comparison of electric shocks taken through the bodies to be examined.

Not until after the invention of the battery and galvanometer was it clearly proved that differences exist between the conducting powers of metals; but by Davy, Becquerel, Ohm, Pouillet, Fechner and others all the fundamental facts were ascertained, and the classical researches of Wheatstone and later of Matthiessen gave us the accurate laws and constants of electrical conduction. By these investigations it was shown that in the case of electric conduction through metallic wires of uniform sectional area their total resistance was proportional to the length, inversely as the cross section, and also proportional to a specific constant for each material called its *resistivity*. Moreover, it was found that this resistivity was considerably affected by temperature, generally being increased in metals by rise of temperature, and decreased for carbon, electrolytic liquids and many badly conducting bodies.

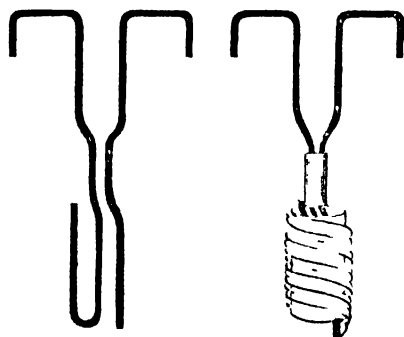


FIG. 1.

Low temperature resistance coil.

Although much knowledge of the behaviour of pure metals and alloys in regard to electric conduction has thus been accumulated, we considered that it would be of great scientific interest to examine with care the changes occurring in the conductivity of these bodies or reciprocally in their resistivity, when cooled to temperatures of two hundred degrees or more below the Centigrade zero by the aid of liquid

oxygen and liquid air.\* Knowing the great influence of very small quantities of impurity on this quality, our first attention was directed to obtaining samples of alloys and metals in a state of great chemical purity, in giving to wires drawn from them a suitable form, and in devising a convenient support or holder by which the electrical resistance of the wire might be measured when immersed in liquid oxygen or liquid air, either in quiet ebullition in an open vessel, or under reduced pressure in a closed one. It will be unnecessary to dwell on the difficulties surrounding the preparation of these accurately drawn metallic wires of pure metal. Suffice it to say that our obligations to Mr. George Matthey, Mr. Edward Matthey, Mr. J. W. Swan and other friends were very great with respect to

\* Almost the only experimental work previously done in this subject seems to have been that of Caillaud and Bouty ('Journal de Physique,' July 1885), on the 'Resistance of Metals at  $-100^{\circ}\text{C}$ ,' using ethylene as a refrigerating agent, and a research by Wroblewski, on the 'Resistance of Copper at very Low Temperatures' ('Comptes Rendus,' 1885, vol. ci p. 161)

this portion of the work. The final outcome of all failures was the production of a resistance coil of the following form:—Two thick wires of high conductivity copper about 3 or 4 mm. thick are bent as shown in Fig. 1, and wrapped round the lower part with a cylindrical sheath of thin vulcanised fibre laced to them by a silk thread. On this sheath, which generally had the form of an oval cylinder, a paraffined silk cord was spirally wound so as to leave a helical groove. In this groove was coiled the resistance wire, of known length and section, and its ends were attached by solder to the ends of the thick copper leads. The wire was wound a little loosely in the groove so as to allow for the great contraction which takes place in cooling, and yet the wire was exposed so as to take up instantly the temperature of the bath, whilst at the same time the mass of material to be cooled down was rendered as small as possible. The length of wire employed was generally about one or two metres, and the diameter from about one-twelfth to half a millimetre ( $\cdot 003$  inch to  $\cdot 02$  inch). Those mean diameters were measured by the microscope micrometer at about fifty to one hundred places for each metre length of the wire. Having thus prepared a great collection of resistance coils of pure metals and alloys, each in the form of a wire of known length and mean diameter, the next operation was the measurement of their resistance at definite temperatures. For the sake of those not

fully familiar with the details of electrical measurement, a moment's digression may be made to explain two of the principal methods in use. Becquerel's work was chiefly conducted with the differential galvanometer. In this instrument two coils of wire of exactly equal length are coiled on one bobbin, in the centre of which hangs a small magnetic needle. The current from a battery (see Fig. 2) divides at one point, and flows along one path through the conductor or conductors under examination and through one coil (No. 2) of the galvanometer. The other portion of the current flows through a wire of variable length called a rheostat, and through the other coil of the galvanometer, equal in every respect to the first coil, but circulates round the needle, N.S., in an opposite direction to that of the current in the first coil. Hence, if the currents are of equal strength the needle is not disturbed at all from its zero position. We can make these currents equal by adjusting the length of the wire of the rheostat so that its resistance is equal to the resistance of the coil

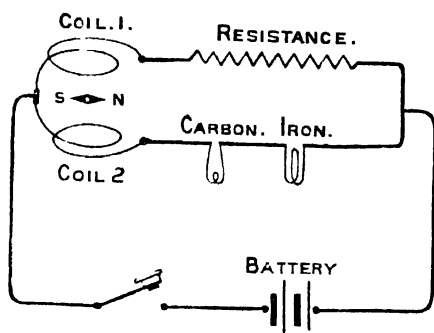


FIG. 2.

Diagram of arrangement of circuits for comparing resistances by means of the differential galvanometer.

being tested. By this means it is easy to verify all the ordinary laws of conduction. We can, for instance, show at once that by cooling an iron wire in iced water its resistance is decreased, whereas in cooling the carbon filament of a glow-lamp its resistance is increased.

This method is not generally so convenient as the arrangement first described by Mr. Hunter Christie to the Royal Society in 1833, re-devised ten years later by Wheatstone in 1843, and which has been always curiously misnamed the "Wheatstone's Bridge," even in spite of Wheatstone's own declaration that he did not invent it.\* In this arrangement (see Fig. 8) the current from a battery B has two paths open to it by which to complete its circuit, and we employ a galvanometer with a single coil to discover two points on these two circuits which are at equal potentials. When these two points are connected the galvanometer needle is undisturbed, and it is a simple matter to show that under these circumstances the numerical values of the electrical resistances of the two segments A X, X D, of the circuit A D, denoted by P and Q, and the resistances R and S which form the

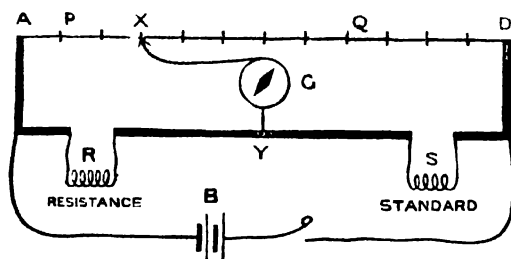


FIG. 3.

Wheatstone's Bridge arrangement for comparing resistances.

other branch, are to one another in simple proportion as R is to S—that is, P is to Q as R is to S. In actual work, one form, useful for lecture purposes, which this arrangement takes is that known as the slide wire bridge (see Fig. 4), and which is before you. In this construction the battery current flows partly through a uniform wire *a b*, stretched over a scale, and partly through a standard resistance *R'*, and the resistance *R* to be tested placed in series with it.

We employ a galvanometer *G* to connect the middle point between *R* and *R'* with some point on the slide wire, and we can always find a point on the slide wire such that no current flows through the galvanometer. The ratio of the unknown resistance *R* is to that of the known standard resistance *R'* in the ratio of the lengths of the two sections into which the contact piece divides the slide wire. Hence *R* is determined in terms of *R'*. Another form of this appliance in which all three arms of the bridge consist of coils of wire capable of

\* See Phil. Trans. 1833, Mr S. Hunter Christie, on the 'Experimental Determination of the Laws of Magneto-Electric Induction.' See also Wheatstone's Scientific Papers, p. 129, 'An Account of several new instruments for determining the Constant of a Voltaic Circuit,' Phil. Trans. vol. cxxxiii. p. 308, 1843.



being joined, as required, in series with each other by plugs, is most commonly used, and it was a most carefully adjusted Elliott bridge of this last pattern which we employed.

All our resistance measurements have been reduced to express them in terms of the International ohm, as defined by the Board of Trade Committee, and obtained by reference to standard coils carefully standardised for us at Cambridge. By this means the whole of our wires were measured at five definite temperatures, viz. at about  $200^{\circ}\text{C}$ .; at the temperature of boiling water,  $100^{\circ}\text{C}$ .; at the temperature of melting ice,  $0^{\circ}\text{C}$ .; at the temperature of solid carbonic acid melting in ether, which gives a temperature of about  $-78^{\circ}\text{C}$ .; and at the temperature of liquid oxygen boiling under a pressure of 760 mm., which gives a temperature of  $-182^{\circ}\text{C}$ .

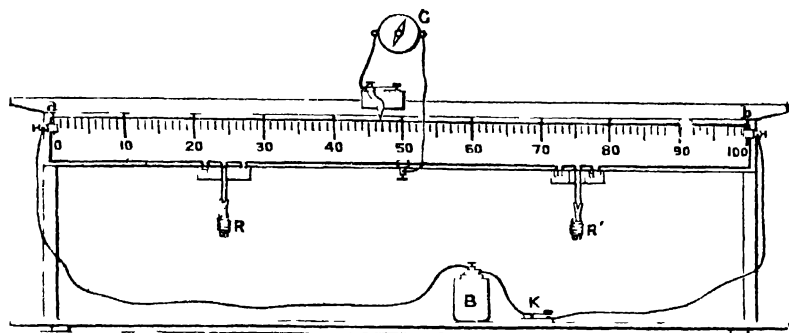


FIG. 4

Slide wire bridge — Lecture form

In this last case the coils were immersed in liquid oxygen contained in suitable vacuum-jacketed vessels. In this connection, I should like to express with due emphasis the opinion that none of this low temperature research would have been possible at all without the assistance of Professor Dewar's most valuable invention the glass vacuum-jacketed silvered vessel. For much of this work it has been necessary to employ many litres of liquid oxygen and air at a time, and to be able to keep it for hours in a state of perfect quiescence and absolutely constant temperature, and in no way could this have been done without this beautiful and scientific device.

Before describing the results of these experiments it may be interesting to exhibit a few of the principal facts. The most striking of them is the very great reduction in electrical resistance, or increase in conductivity, experienced by all the pure metals when cooled in liquid air. Here, for instance, are two coils of iron wire: balancing them on the bridge we find them to be of exactly equal resistance, but if one of the coils is cooled in liquid air its resistance is reduced to about one-tenth of its resistance at the ordinary tem-

perature of the air. We may also compare the resistances of these two similar iron coils, when one is placed in boiling liquid air and the other in boiling water. The resistances, instead of being in the ratio of one to one, are now in the ratio of one to twelve. Again, if we take two wires, one of pure iron and one of pure copper, of exactly equal length and equal section, we find that at ordinary temperatures ( $15^{\circ}\text{C.}$ ) the iron wire has about six times the resistance of the copper; but if we cool down the iron wire in liquid air to  $-186^{\circ}\text{C.}$ , still keeping the copper coil at the ordinary temperature ( $15^{\circ}\text{C.}$ ), we now find that the iron coil has actually become a much better conductor (about 30 per cent. better) than the copper.\* On the other hand, if we examine the behaviour of this coil of German silver, which is a copper-zinc-nickel alloy, or of this platinum-silver coil, we find that the cooling down through  $200^{\circ}$  has a comparatively small effect upon its electrical resistance. We thus see that whilst pure metals have their electrical resistance immensely decreased by cooling to the temperature of liquid air, alloys generally do not experience anything like so great a change.

A word or two must next be said on the manner in which we have represented graphically all the results of our experiments. We desired to delineate lines on a chart so as to express the change in specific resistance of all our metals and alloys in terms of temperature; and the question then arises, how was the temperature measured? You already know that an ordinary thermometer, whether mercury, alcohol, or air, would be useless to measure temperatures at which even air liquefies under ordinary pressures.

The employment of the constant pressure hydrogen thermometer with reduced pressure would have given us temperature readings very approximately those of the absolute thermodynamic scale, but the experimental difficulties of its use would have been enormous. We preferred to use the platinum resistance thermometer, and to express our temperatures in platinum degrees as follows:—Our experience has shown us that a pure soft annealed platinum wire may be cooled as often as necessary to the lowest attainable temperatures, and yet will always have the same resistance when measured again at other constant temperatures. Availing ourselves of this fact, we have used in all this work a low temperature platinum thermometer made in the following way:—A well-annealed platinum wire is made into a resistance coil, as already described. Its resistance is carefully measured at the temperature of boiling water,  $100^{\circ}\text{C.}$ , and melting ice,  $0^{\circ}\text{C.}$  From these measurements we construct a scale of temperature as follows:—A horizontal line A E (see Fig. 5) is taken on which to mark off temperature, and any two points A and B are taken on this line and the length A B divided into one hundred equal parts. At these points B and A perpendiculars are set up

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\* The exact resistances of the coils used for the experiment were as follows: Iron at  $16^{\circ}\text{C.}$  =  $7.003$  ohms, and reduces to  $0.711$  ohms; at  $-186^{\circ}\text{C.}$  copper at  $16^{\circ}$  =  $1.169$  ohms, reduces to  $0.2033$  at  $-186^{\circ}\text{C.}$

proportional to the resistance of the platinum wire at  $0^{\circ}\text{C.}$  and at  $100^{\circ}$  respectively, and through the tops of these perpendiculars a sloping straight line is drawn until it cuts the axis of temperature at E. The graduation of the horizontal line is continued in both directions on the same scale as the subdivision of the line between the points marked 0 and 100. To measure and define any other temperature, say, for instance, the boiling-point of liquid oxygen under a pressure of 760 mm., we have simply to measure the resistance of the platinum wire in the liquid oxygen. We then look out on the chart the ordinate which has the same numerical value as the resistance of the wire in the oxygen, and at the foot of that ordinate

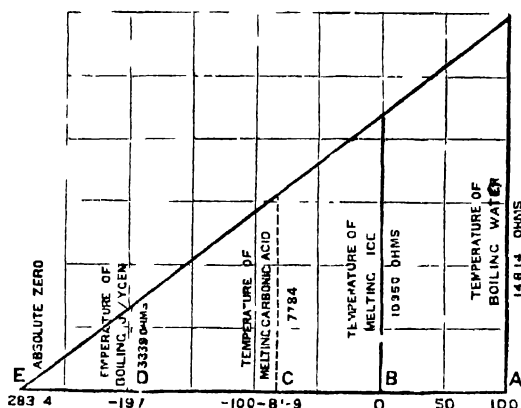


FIG. 5.

Method of constructing a scale of platinum temperature.

we find a number, viz. ( $-197$ ), which is the temperature of the liquid oxygen on this platinum scale.

Two questions then arise—first, Do all annealed platinum wires give, when used in this way, the same numerical values for definite and identical temperatures? The answer to this is, Nearly, but not quite. In the case of two thermometers much used by us, the difference was about half a degree at  $-100^{\circ}\text{C.}$ , the boiling-point of liquid ethylene. Into this matter it is not possible here to enter more fully; suffice it to say that we have invariably referred our temperature measurements to one standard thermometer. The second question is equally important—it is, What is the relation of the scale of temperature so defined to the absolute thermodynamic scale; or, which is very nearly the same thing, to the scale of temperature defined by a constant pressure hydrogen thermometer? If the air thermometer and platinum thermometer readings are made to agree at  $0^{\circ}\text{C.}$  and  $100^{\circ}\text{C.}$ , then a temperature which would be called  $50^{\circ}$  on the Centigrade scale would be denoted by  $50.4$  nearly on the platinum thermometer scale, and corresponding to  $-78^{\circ}$  on the Centigrade scale, which is the temperature of carbonic acid melting in ether.

The platinum temperature by our standard is  $-81^{\circ}\cdot9$ ; and corresponding to  $-182^{\circ}$  C., which is very nearly the Centigrade temperature of liquid oxygen boiling at the normal pressure of 760 mm.; the platinum temperature by the same standard is  $-197^{\circ}$ . The conversion of these numbers representing low temperatures in platinum degrees into the numbers representing the corresponding absolute thermodynamic temperatures is a work we have reserved for a future research;\* but meanwhile it may be said that there is no method of measuring low temperatures which is so easy of application and so accurate as that depending on the use of a platinum thermometer. All our work has been ultimately referred to one standard platinum thermometer, which we call  $P_1$ .

A suggestion may here be made. There is no reason why the Board of Trade electrical laboratory should not possess a standard platinum thermometer defining officially platinum or absolute temperatures for all time, and with which other platinum thermometers could be easily and very accurately compared.

Having thus defined our scale of temperature, we proceeded to embody the whole of our results in a chart which is now before you (see Fig. 6), and in which vertical distances represent resistivity, or specific resistance, or the resistance in absolute measure per cubic centimetre of the various metals, and horizontal distances represent platinum temperatures. The curves indicate the manner in which the resistivity varies with temperature for each substance.

The values of the resistivity of most ordinary metals and alloys are given in the table adjoining:—

ELECTRICAL RESISTIVITY OF PURE ANNEALED METALS

Metal.	Resistivity in C.G.S. units at $0^{\circ}$ C	Percentage increasement, $0^{\circ}$ to $100^{\circ}$ C	Atomic volume.
Silver .. .. .	1,468	40·0	10·04
Copper .. .. .	1,561	42·8	7·10
Gold .. .. .	2,197	37·7	10·04
Aluminium .. .. .	2,665	43·5	10·56
Magnesium .. .. .	4,355	38·1	13·76
Zinc ... .. .	5,751	40·6	9·12
Iron .. .. .	9,065	62·5	7·10
Cadmium .. .. .	10,023	41·9	12·96
Palladium .. .. .	10,219	35·4	9·12
Platinum .. .. .	10,917	36·69	9·12
Nickel .. .. .	12,323	62·2	6·94
Tin .. .. .	13,048	44·0	16·20
Thallium .. .. .	17,633	39·8	17·20
Lead .. .. .	20,380	41·1	18·27
Mercury .. .. .	94,070	38·88	14·56
Bismuth .. .. .	108,000	—	21·43

\* Callendar has shown that over a wide range of temperature from  $0^{\circ}$  C. to  $700^{\circ}$  C. the difference between the platinum temperature and the air thermometer temperature is a parabolic function of the absolute temperature.

## ELECTRICAL RESISTIVITY OF ALLOYS.

Alloy.	Composition.	Resistivity in C.G.S. units at 0° C.	Percentage increment, 0° C. to 100° C.
Aluminium-copper .. .. .	94 : 6	2,901	38.1
Aluminium-titanium .. .. .	—	3,887	29.0
Aluminium-silver .. .. .	94 : 6	4,641	23.8
Gold-silver .. .. .	90 : 10	6,280	12.4
Copper-aluminium .. .. .	97 : 3	8,847	8.97
Copper-nickel-aluminium .. .. .	87 : 6½ : 6½	14,912	6.45
Platinum-rhodium .. .. .	90 : 10	21,142	14.3
Nickel-iron .. .. .	95 : 5	29,452	20.1
German silver .. .. .	= Cu <sub>8</sub> Zn <sub>2</sub> Ni <sub>2</sub>	29,982	2.73
Platinum-iridium .. .. .	= Pt <sub>4</sub> Ir	30,896	8.22
Platinum-silver .. .. .	1 : 2 = PtAg <sub>2</sub>	31,582	2.43
Platinoid .. .. .	—	41,731	3.1
Manganin .. .. .	—	46,678	0.
Iron-manganese .. .. .	88 : 12	67,148	12.7

The first thing which strikes us on looking at the chart (Fig. 6) is that the lines for the pure metals all converge downwards in such a manner as to indicate that their electrical resistance would vanish at the absolute zero of temperature, but that no such convergence is indicated in the case of alloys. We have found that the slightest impurity in a metal changes the position of the resistance line. In the next place, note that the order of conductivity is different at low temperatures to that at ordinary temperatures. At 13° C. pure silver is the best conductor, but at -200° pure copper is better than silver, and the position of mercury is, of course, very different.

Again, the lines of some metals are very much curved. The principal magnetic metals, iron and nickel, have lines which are very concave upwards, and this is a characteristic apparently of many magnetic alloys. The mean temperature coefficient of these magnetic metals between 0° C. and 100° C. is much larger than that of other metals, and the percentage decrease in resistance in cooling them from + 200° C. to -200° C. is greater than in the case of any other metal. It is worth noting in passing that these magnetic metals, iron and nickel, have smaller atomic volumes than any other metal, and that, generally speaking, the worst conductors amongst the metals are those that have the large atomic volumes and large valency.

Next turning to alloys, we may make mention of a few general facts with regard to their resistance. If to one pure metal we add a small quantity of any other metal the result is always to raise the resistance line almost parallel to that of the predominant constituent. Thus, in our own chart, the alloy consisting of 6 per cent. of copper with 94 per cent. of aluminium is parallel to the aluminium line, but higher up. Three per cent. of aluminium added to 97 per cent. of copper yields an alloy with a resistance line parallel to that of

copper, also higher up. When two pure metals are alloyed together in various proportions there is generally some proportion in which the resultant alloy has a maximum resistivity, and except in the case of alloys of zinc, tin, lead and cadmium with each other, the resistivity of the alloy is greater than that of either of its constituent metals. In the case of many well-known alloys the proportions which give high, if not the highest resistivity are those which correspond to definite and possible chemical combinations of the metals with each other, as, for instance, in the well-known platinum-silver alloy in proportion 33 to 66, which corresponds in proportion with the combination  $\text{PtAg}_4$ ; the iron-nickel alloy in proportion of 80 to 20, which corresponds with the combination  $\text{NiFe}_4$ ; the platinum-iridium alloy 80 to 20, which corresponds with the combination  $\text{IrPt}_4$ ; and the copper-manganese alloy 70 to 30, which corresponds with the compound  $\text{Cu}_2\text{Mn}$ ; all of which are, as far as valency is concerned, possible compounds. It is, however, found that very high resistivity generally involves in alloys a want of tenacity and ductility, and when we reach such limits as 100 microhms per cubic centimetre we begin to find the solid alloys becoming less useful on account of this deterioration of their useful mechanical quality.

We have especially studied the electrical resistance at low temperatures of a large series of steel alloys containing varying proportions of nickel, aluminium, chromium, tungsten and manganese in them.

We have found that the electrical effect of adding to the iron the other elements of the alloy is usually to shift up the resistance line nearly parallel to itself, so that the resistance lines of all the iron alloys are nearly parallel to that of the iron line, only the absolute value of all the ordinates is increased. This is equivalent to saying that the effect of the added material is to increase the specific resistance, but not to alter the slope or form of the resistance curve. Amongst these steel alloys there are two or three that are very interesting. A nickel-steel alloy containing 19 per cent. of nickel, sent to us by Mr. R. A. Hadfield, exhibits some very extraordinary properties. Nickel-steel alloys with large percentages of nickel can, as Dr. Hopkinson has shown,\* exist over wide limits of temperature in two different physical states, in one of which they are strongly magnetic and in the other of which they are feebly magnetic, and they pass from the non-magnetic to the magnetic on cooling to low temperatures. Here, for instance, is a sample of the 19 per cent. nickel-steel in the non-magnetic condition. If it is cooled in liquid air we can make it pass instantly into a magnetic condition. In the first state it is fairly ductile and plastic, but in the second state it is very hard and brittle. Moreover, its electrical resistance and thermoelectric power are both permanently altered on undergoing this change. In the non-magnetic state it has a high resistivity of about

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\* See Proc. Roy. Soc. 1890, vol. xlvii. p. 138.

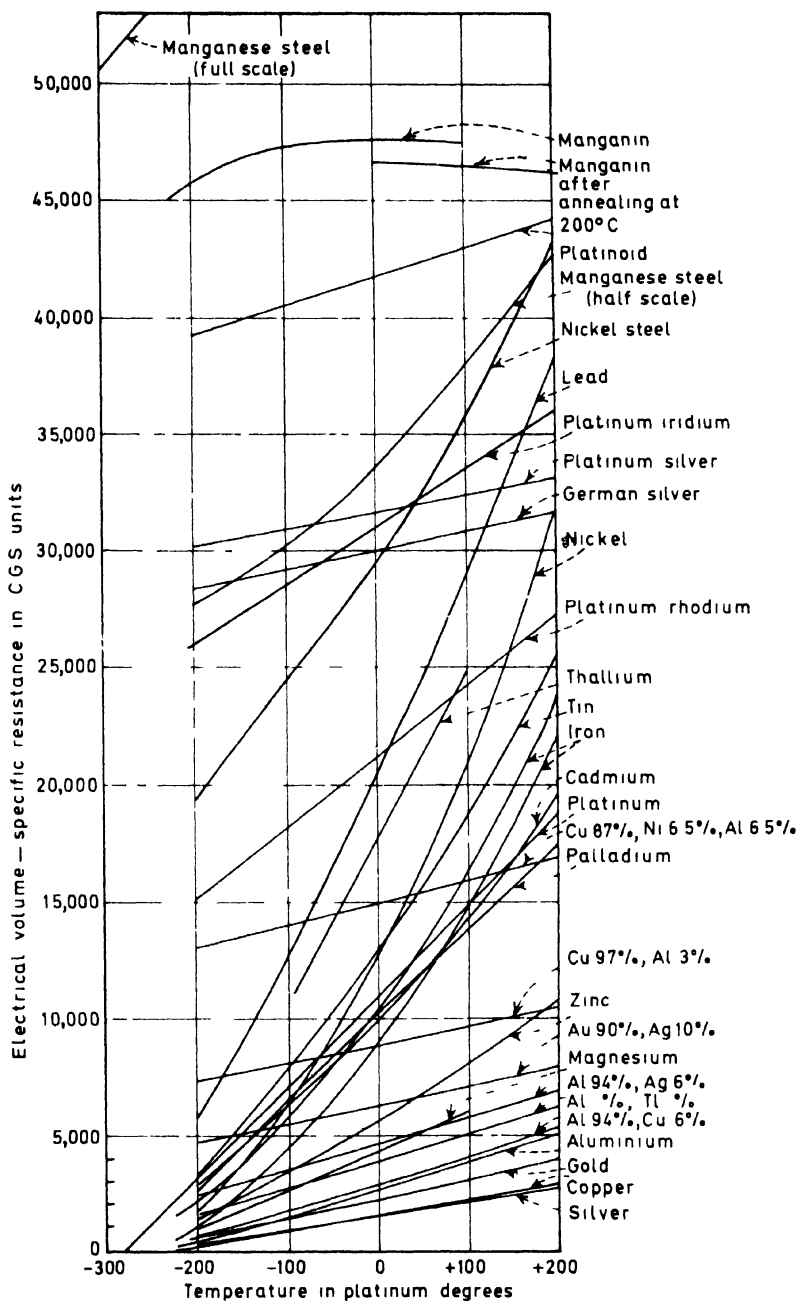


FIG 6

Chart showing the relation of Electrical Resistivity and Temperature

81,500 C.G.S. units per cubic centimetre at  $0^{\circ}$  C., but on cooling in liquid air and becoming magnetic it is found to have decreased to about 47,200 C.G.S. units when taken at  $0^{\circ}$  C. A very pretty way of showing this difference in resistivity is to dip one half of a wire of the 19 per cent. nickel-steel in liquid air, and then take it out, and pass a strong electric current through the wire. The current raises the half which has not been dipped into liquid air to a red heat before the other half is visibly red hot.

It is, perhaps, more correct to say that this alloy can exist in an infinity of different physical states, because we have found that the lower it is cooled in temperature the lower its resistivity can be made

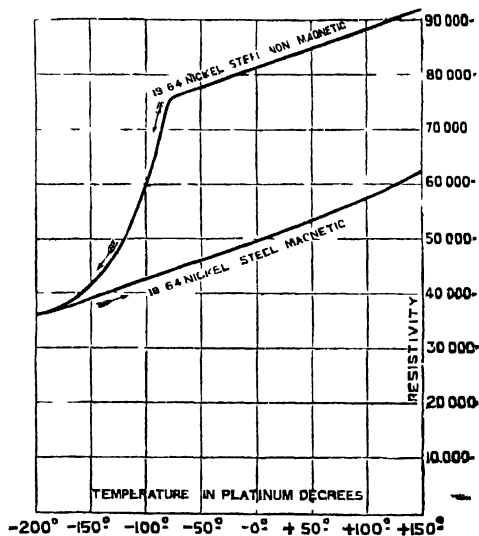


FIG. 7

Curve showing the Variation of resistivity of nickel-steel (19.64 per cent nickel) when taken through a cycle of temperature from  $+150^{\circ}$  to  $-200^{\circ}$  and back again.

to be when measured again at ordinary temperatures. On heating up the alloy again to a bright red heat it goes back into the non-magnetic ductile state.

The chart (Fig. 7) before you shows the manner in which the electrical resistance varies between the limits of  $-200^{\circ}$  C. and  $150^{\circ}$  C. when the alloy is taken through a cycle of temperature beginning at  $150^{\circ}$  C. in its non-magnetic state.

The 29 per cent. nickel-steel exhibits the same characteristics in a less marked degree. A close study of this interesting material shows that there is room for much valuable work upon it yet.

A manganese-steel, brought to notice by Mr. R. A. Hadfield, having about 12 per cent. of manganese in it, is also capable of existing in two states, a magnetic and a practically non-magnetic variety.



The magnetic variety, which is much more brittle, is, however, in this case formed by the prolonged slow heating of the non-magnetic variety out of contact with air. In the non-magnetic condition the material has a rather high specific resistance at  $0^{\circ}\text{C.}$ , about 65,700 C.G.S. units per cubic centimetre; but the magnetic variety has a much lower specific resistance, viz. about 51,400 C.G.S. units at  $0^{\circ}\text{C.}$

In all these cases it is interesting to note that the change of the alloy into the magnetic variety is accompanied by a decrease in resistivity or increase in conductivity, and an increase in brittleness.

We have tried cooling this non-magnetic variety of manganese-steel in liquid air, but have not been able in that way to make any change in its condition as regards magnetic susceptibility.

There is a particular alloy, of copper 84 per cent., manganese 12 per cent., and nickel 4 per cent., called manganin, which at ordinary temperature exhibits but little change of resistance with change of temperature. On taking the curve of its resistance over wide ranges of temperature we find that its curve is very concave downwards, and the vertex of the curve lies at about  $16^{\circ}\text{C.}$  Hence at ordinary temperatures small changes of temperature make no change in its resistance; but above that point its temperature coefficient is negative, and below it it is positive. All alloys in which a negative temperature coefficient has been observed are probably instances of the same mode of variation of resistance. It may be noted in passing that the element manganese when present in an alloy seems to have a great tendency to produce high resistivity and small temperature coefficient.

Returning then to the pure metals, we may ask, What is the meaning of the fact that in their case the resistance lines all converge so as to indicate that the electrical resistance would vanish at the absolute zero of temperature?

We know that the passage, as we call it, of an electric current through a conductor heats it, and that by Joule's law the rate of production of heat in the conductor is proportional to the square of the current strength and to the total resistance of the conductor.

Suppose we take two wires, say of iron and a certain copper-nickel-aluminium alloy having the same resistivity at  $100^{\circ}\text{C.}$  and of the same size and length. These wires will at  $+100^{\circ}\text{C.}$  have the same resistance. A given current flowing through them will therefore generate heat in them both at the same rate.

Cool them both down, however, to the temperature of liquid air. In the case of iron-wire the resistance is reduced to one-fifteenth of its value at  $-200^{\circ}\text{C.}$ , in the other case it is reduced by only 10 per cent. Hence, at the low temperature the alloy dissipates energy for the same current  $13\frac{1}{2}$  times as rapidly as the pure metal.

It is a logical deduction from all we know to conclude that if we could reach the absolute zero of temperature the pure metal would not dissipate the energy of the current at all. Imagine two iron wires, then, stretched through space, say from the earth to the moon,

and kept everywhere at the absolute zero of temperature, we could transmit any amount of electrical energy along them without dissipating any of it as heat in the wires.

As a consequence of this, any pure metal cooled to the absolute zero of temperature would become a perfect screen for electro-magnetic radiation, and would be perfectly impenetrable to electro-magnetic induction.

We can show this increase in the power of electro-magnetic

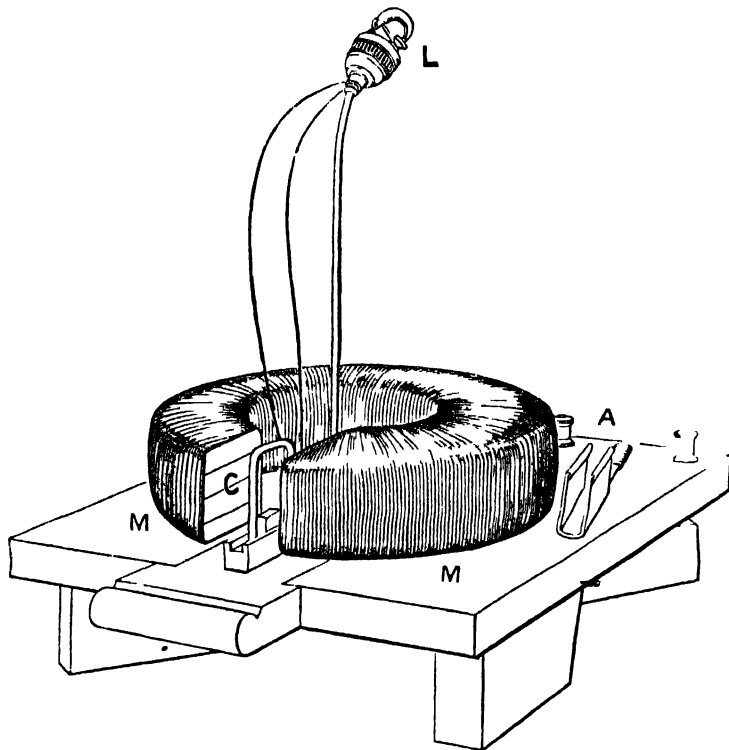


FIG 8

An alternating current magnet having a coil C between its poles over which a shield A of aluminium can be placed

screening by metals when cooled in the following way. A suitable coil of wire C is placed (see Fig. 8) between the poles of an alternating current magnet M, M and a small incandescent lamp L connected with the coil. When the magnet is excited it induces currents in the coil and the lamp glows up. A cap of aluminium A is made of such a size as to drop easily over the coil. This aluminium is not of sufficient thickness or conductivity to screen off the induction when it is warm. If, however, we cool the aluminium cap in liquid air and

then drop it over the coil the lamp for one instant goes out, but it brightens up again as the metal cap instantly warms up. This shows us, however, that if the cap were at the absolute zero of temperature it would then be a complete screen for the induction. In fact, these experiments furnish us with a new definition of what we mean by the absolute zero of temperature. It is the temperature at which perfectly pure metals cease to have any electrical resistance.

In the conduction of currents at ordinary temperatures as we generally know it, two effects are inseparably connected with the conveyance of energy by this process. One is the dissipation of some of the energy as heat in the conductor, the other is a loss of potential or fall of electric pressure, the latter being one of the factors in the equivalent of the energy so dissipated. If, however, the conductor is at the absolute zero of temperature, there would be no heat produced in it, and no fall of potential along it, either for large or small currents. What then under these conditions is the function of the conductor? The answer is, that it becomes a mere boundary serving to limit the electro-magnetic field and determine the direction in which the energy transmission is taking place. These experiments therefore may be regarded as forging one more important link in that chain of experimental evidence which compels us to look for the processes concerned in the conveyance of energy by an electric current, not inside the conductor as we call it, but in the dielectric or medium outside. We may then ask, How is it that different bodies have such various dissipative powers when acting in this way as the boundary of an electro-magnetic field? The only suggestion on this point I venture to make here is as follows:—Materials of high specific resistance have all probably a very complex molecular structure. The alloys of high resistivity are probably not merely solidified mechanical mixtures of metals, but chemical compounds, and even in the case of elementary bodies like carbon and sulphur, which have high resistivity, these last-named bodies may have, owing to their high valency and tendency of their atoms to auto-combination, a complex molecular structure.

This structure may bestow upon them the power of taking up energy from the electro-magnetic medium, just as gases with a highly complex molecular structure are very absorbent of radiant heat, which, if the electro-magnetic theory of light is true, is only another form of electro-magnetic energy. All we know at present about the processes at work during the time a conductor is traversed by an electric current, is that there is a magnetic field outside the conductor and also within the mass of the conductor, and that some mechanism is at work absorbing energy through the surface of the conductor and dissipating it as heat in the interior. The resistance of a conductor is best defined as, and numerically measured by, the number expressing the rate at which it dissipates electro-magnetic energy per unit of current. For the same current, that is for the same external

magnetic field, conductors dissipate this energy at very different rates. Some, like silver and copper, which have the lowest rates, are elements of low valency and relatively small molecular volume, and have probably a simple molecular structure; others, like alloys of high resistivity, have in all probability a more complex molecular structure. Both this last, as well as the molecular mobility charac-

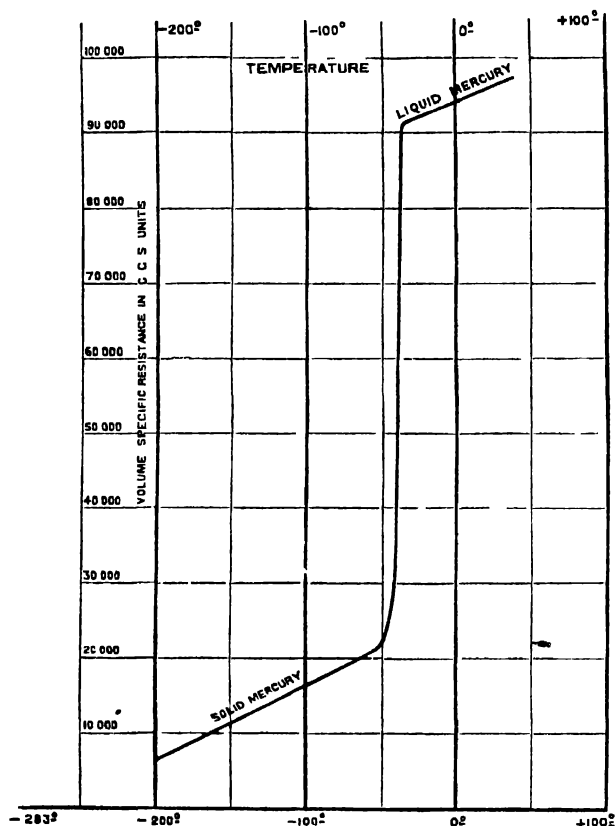


FIG. 9

Resistivity curve of mercury in terms of platinum temperature.

teristic of the liquid state, are conditions which bestow the power of taking up rapidly and dissipating the energy of the electro-magnetic field, and this energy has to be kept supplied from external energy-transforming sources. We cannot, however, at present profitably construct further mechanical hypotheses to account for this difference between conductors, in the presence of our great ignorance about ether, molecules and energy.

In passing from the liquid to the solid state there is generally an immense increase in the conducting power of metals. This is well

shown in the case of mercury. A glass tube a metre in length was formed into a spiral coil and filled with pure mercury, suitable connections being provided at the ends. This coil was imbedded in a mass of paraffin wax, and a platinum wire thermometer placed in contact with it. The whole mass was then reduced to the temperature of liquid air, and observations taken of the resistance of the mercury as it heated slowly up after being removed from the liquid air. The curve in Fig. 9 shows the manner in which the resistance increases with great suddenness between  $-41^{\circ}$  and  $-36^{\circ}$  as the metal passes into the liquid condition. The resistance becomes four times greater between  $-50^{\circ}$  and  $-36^{\circ}$  in the course of  $14^{\circ}$  rise of temperature, and whilst in the act of passing through the melting

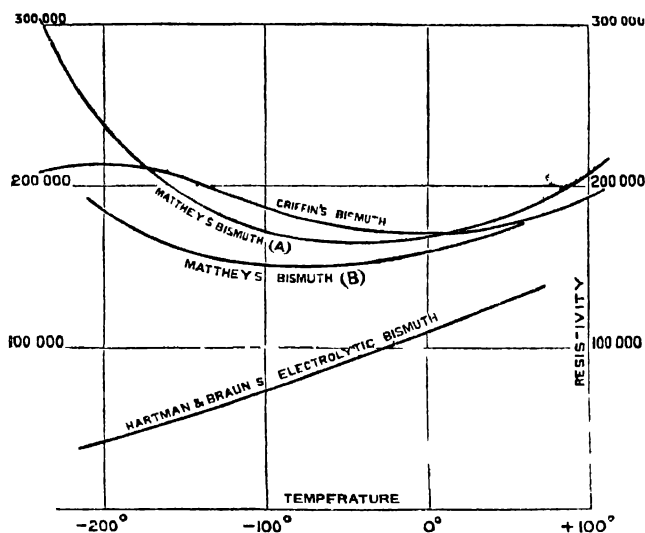


FIG. 10

Resistivity curves of bismuth in terms of platinum temperature

point of the mercury at  $-38^{\circ}.8$  C. This chart shows that the resistance curve of the mercury in the solid state tends downwards, so as to indicate that its resistivity would completely vanish exactly at the absolute zero of temperature. It is interesting to note also that the portion of the resistance curve belonging to mercury in the solid state is sensibly parallel to that portion of it in the liquid state.

We carried on a long struggle with bismuth in the endeavour to unravel some of the electrical peculiarities of that metal at low temperatures. Chemists are aware of the extreme difficulty of preparing bismuth in a state of perfect chemical purity by purely chemical means. From several different sources we procured bismuth which had been carefully prepared by the reduction of the oxychloride or nitrate after careful re-precipitations. This bismuth was then pressed into wire, and its resistance curves taken down to the lowest attainable

temperatures. We found some very extraordinary results. Although sensibly agreeing in resistivity at ordinary temperatures, in two cases (see Fig. 10) the resistance curves had a minimum point, and after reaching this at about  $-80^{\circ}$  tended upwards again; thus showing that the resistance was increasing as the metal was further cooled. These curves could be repeated as often as necessary with these samples. Another specimen gave a curve with a double bend (see Fig. 10). These results convinced us that it would be necessary to prepare bismuth electrolytically, and with the assistance of Messrs. Hartmann and Braun, of Frankfort, who have made a special study of the preparation of electrolytic bismuth, we were provided with a quantity of the metal which examination showed to be chemically pure. On taking the resistance curve of a sample of this electrolytic bismuth when pressed into uniform wire under great pressure, we found that its behaviour was perfectly normal, and that the resistance line tended downwards, as in the case of all other pure metals, to the absolute zero. Also we found that the specific resistance of this last is very much less than that of the chemically prepared samples, and less even than that employed by Matthiessen. Hence pure bismuth is no exception to the law enunciated above. Bismuth is characterised especially by many peculiarities. It has been known for some time that the resistance of a bismuth wire is increased when it is placed in a magnetic field, so that the lines of the field are perpendicular to the direction of the current flow. This is easily shown by means of one of Hartmann and Braun's spirals, manufactured now purposely for measuring magnetic fields.

We have, however, discovered that if bismuth is cooled to the temperature of liquid air the effect of any given magnetic field in changing its resistance is increased many times. Thus, for example: A certain bismuth wire we used had a resistance of  $1\cdot690$  ohms at  $20^{\circ}$  C. Placed in a magnetic field of strength 2750 C.G.S. units so that the wire was transverse to the direction of the field, its resistance was increased to  $1\cdot792$  ohms, or by six per cent. The wire was then cooled in liquid air and its resistance lowered to  $0\cdot572$  ohms. On putting it then into the magnetic field of strength 2750 C.G.S. units its resistance became  $2\cdot68$  ohms. Hence it had increased 368 per cent. This magnetic field can thus actually reverse the effect of the cooling, and cause the bismuth, when cooled and magnetised, to have a greater resistance than when at ordinary temperatures and unmagnetised. We are at present engaged in further unravelling the problems presented by this new discovery with regard to bismuth.\* It is certainly very startling to find that a magnetic field which increases the resistance only 5 per cent. at ordinary temperatures increases it five times at  $-186^{\circ}$  C. We have recently discovered a similar,

\* Since the delivery of this discourse we have been able, by the employment of a powerful electro-magnet kindly lent to us by Sir David Salomons, to increase the resistance of bismuth, when cooled in liquid air, more than 150 times, by magnetising it transversely in a field of 22,000 C.G.S. units.

but much smaller effect in the case of nickel longitudinally magnetised. It will be seen that this process of taking the resistance of a conductor in liquid air is one which affords us a very critical means of discrimination as to the chemical purity of a metal. It ranks almost with the spectroscope as an analytical method. There is one other method by which we can exhibit the change in conductivity in a metal when cooled, and that is by the increased deflection which a disc of the metal experiences when suspended in an alternating current field in such a position that the plane of the disc is at an angle of  $45^\circ$  to the direction of the field.

Time will only permit one brief reference to the behaviour of carbon in regard to electrical conductivity when cooled to low temperatures. We have found that carbon in the form of carbon filaments taken from various incandescent lamps continued to increase in resistance as it was lowered in temperature. The resistivity at various temperatures of the carbon from an Edison-Swan lamp is as follows:—

C.G.S. Units.	Temp. C.
$3835 \times 10^3$ at	$99^\circ$
$3911 \times 10^3$ at	$18^\circ.9$
$3953 \times 10^3$ at	$1^\circ$
$4054 \times 10^3$ at	$-78^\circ$
$4079 \times 10^3$ at	$-100^\circ$
$4180 \times 10^3$ at	$-182^\circ$

These values, when represented on a chart, give almost a straight line, and show that the resistivity of carbon continually increases as it is cooled, but at a very slow rate. Its temperature coefficient is therefore negative, and of about the same absolute magnitude as many alloys of high resistivity. The resistivity of this form of carbon is about three thousand times that of silver. Adamantine carbon taken from a Woodhouse and Rawson lamp had a resistivity 50 per cent. greater.

All the so-called insulators—e. g. glass, gutta-percha, ebonite, paraffin—have resistivities enormously greater than that of carbon, but like it, their resistance increases as the temperature is lowered. For the sake of comparison we have placed upon this chart of lines of metallic resistivity (referring to the large diagram used at the lecture) the resistance line of carbon with ordinates drawn to a scale of one-hundredth part of those of the metals. To properly represent to the full scale the line of carbon, this chart, which is 15 feet long, would have to be made one-third of a mile long. If we desired to represent on the same scale the resistivity of gutta-percha, the length of the chart would have to be billions of miles—in fact, so long that light would take 5000 years to traverse it from one end to the other; even then, to represent to the same scale the resistance lines of paraffin and ebonite, it would have to be thirty or forty times longer.\*

We must next pass on to consider some problems in thermo-

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\* The resistivities of platinoid, carbon, and gutta-percha at  $0^\circ$  C. are nearly in the ratio of the numbers  $4 \times 10^4$ ,  $4 \times 10^6$  and  $4 \times 10^{23}$ .

electricity which have engaged our attention. If we construct a thermo-electric couple of two metals and connect this with a galvanometer, and if one junction is kept at a constant temperature, say  $0^{\circ}\text{C}$ ., whilst the other junction is heated or cooled to various temperatures, we shall in general, but not always, find an electromotive force acting in this circuit when the junctions are at different temperatures. This electromotive force depends on three things—the nature of the metals, the temperatures of the junctions, and on a certain temperature called the *neutral temperature* of the metals. An important matter in the experimental study of thermo-electric action is to discover the position of these neutral temperatures, when different metals are tested with lead as the standard of comparison, and when one junction is kept at  $0^{\circ}\text{C}$ . Elaborate experiments made by Professor Tait many years ago furnished full information on this matter for temperatures lying above  $0^{\circ}\text{C}$ ., and we especially desired to extend this knowledge to ranges of temperature between  $0^{\circ}\text{C}$ . and  $-200^{\circ}\text{C}$ . Accordingly, a number of thermo-electric junctions were prepared of various pure metals and alloys, the comparison metal

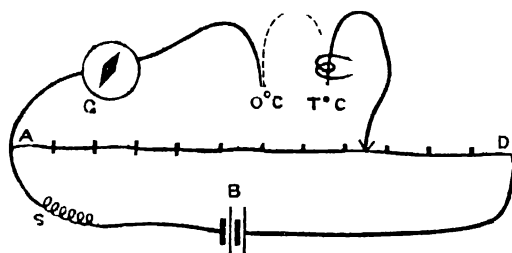


FIG. 11.

Potentiometer arrangement for measuring thermo-electromotive forces.

being always pure lead. These couples were grouped together, and one set of junctions always kept at  $0^{\circ}\text{C}$ . in melting ice. The other set of junctions was cooled to various low temperatures by means of liquid air. The experimental process then consisted in measuring the electromotive force set up in each couple respectively, and at the same instant measuring the temperature of the low temperature junction. After various failures a device was adopted for making this double measurement with great accuracy and expedition.

The arrangement consisted of a combined potentiometer and resistance balance (see Fig. 11). A long uniform wire stretched over a scale had a battery connected to its two ends so as to make a fall of potential down the wire which could be regulated by appropriate resistances. It will be easily seen that we can combine a galvanometer and resistance coil with this arrangement in such a manner as to form it into a Wheatstone's bridge or a potentiometer. In this latter form of instrument an unknown electromotive force is balanced



against the known fall of potential down a certain length of a graduated wire, and a galvanometer employed to ascertain the point on the slide wire at which this is the case. Omitting details, it may be stated that I succeeded in devising an arrangement of circuits in which this change from a potentiometer to a resistance bridge was effected by moving two brass plugs from one pair of holes to another. This instrument formed a most useful combined resistance and electromotive force measurer which enabled us to do two things—first, to measure the electromotive force in any thermo couple; secondly, to measure the temperature of the low temperature junction by measuring the resistance of a platinum wire wound round that junction and acting as a thermometer. In actual practice the platinum thermometer consisted of a small hollow copper cylinder, in the interior of this cylinder being inserted a number of the thermo junctions, and round the outside of which the platinum thermometer wire was wound. Aided

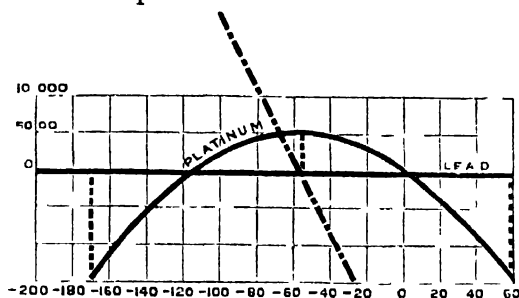


FIG. 12.

Curve of thermo-electromotive force of a platinum-lead couple at various temperatures; one junction kept at  $0^{\circ}\text{C}$ ., the temperature of the other being varied. The sloping dotted line represents the variation of the thermo-electric power of platinum with respect to lead.

by this device we were able to measure temperatures with an accuracy of  $\frac{1}{25}$  of a degree at a temperature of  $-200^{\circ}\text{C}$ ., and to ascertain at the same instant the exact electromotive force acting in the couple. When these arrangements had been perfected the method adopted was to put one set of the junctions in melting ice. The other set, enclosed in the copper cylinder, were imbedded in a mass of paraffin wax, which was then cooled down to the temperature of liquid air. The mass was then removed and inserted in a vacuum vessel, and allowed to heat up very slowly. At frequent intervals during the heating the electromotive force of the couple was taken, and also the temperature of the junction.\*

The events which under such conditions happen in the case of a platinum-lead junction can easily be shown and are very interesting (see Fig. 12). At the first immersion of one junction in liquid air, whilst the other is in melting ice, we get a current as shown by the

\* For fuller information see Dewar and Fleming on the 'Thermo-Electric Powers of Metals and Alloys,' 'Philosophical Magazine,' July 1895.



galvanometer in one direction. On lifting one junction out of the liquid air it begins to warm up. The first effect of this heating is to reduce the thermo current in the circuit. At about  $-111^{\circ}$  on our platinum scale, some distance therefore above that of liquid air, the current in the circuit falls to zero. As the junction continues to heat up, the current increases again in the opposite direction. At about  $-60^{\circ}$  the low temperature junction reaches the temperature called the neutral temperature, and the current is a maximum in one direction. It then begins to fall off once more, and finally becomes zero again when the two junctions are both at the temperature of melting ice, and it lastly increases in the opposite direction as this variable junction continues to warm up from  $0^{\circ}$  C. to higher temperatures.

Having carried out the observations described with all our thermo couples, the results were represented on a chart (see Fig 13) as follows:—A horizontal line was taken on which were marked off divisions representing platinum temperatures. Vertical ordinates were then drawn at various temperatures for each couple, representing the electromotive force in this couple when the cold junction was at the temperature denoted by the abscissa of that ordinate. In this way a series of curves were delineated which all passed through the point representing  $0^{\circ}\text{C}$ . These curves are the curves of thermo-electromotive force.

In Professor Tait's researches on this subject he adopted a method of representing the facts which has many advantages. Suppose the couple to have one junction at a constant temperature and the other to be varying. At any instant the electromotive force of the couple is varying at a certain rate with the changing temperature of the non-constant junction. This rate measures what is called the

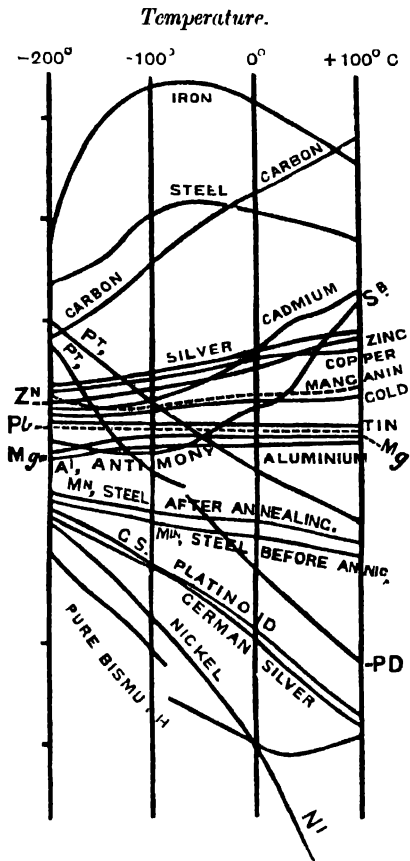


FIG. 14.

Curves showing the variation with temperature of the thermo-electric power of various metals. The thermo-electric line of lead being represented by the dotted line.

*thermo-electric power* of the metals with respect to each other at that temperature. If we measure the slope of the electromotive force curve at any point, it can easily be shown that the numerical value of this slope gives us the rate of change of electromotive force with temperature. If we plot these slopes in terms of the corresponding temperature, we obtain another set of curves called curves of thermo-electric power. Lead is always taken as the standard metal for comparison, because the Thomson effect in lead is zero. From our chart of thermo-electromotive forces we have constructed another one of thermo-electric powers (see Fig. 14). The lines of thermo-electric power cut the lead line in various places, and the temperature at which they do this is called the neutral temperature of that metal with respect to lead. Professor Tait deduced from his experiments that these thermo-electric lines were straight lines for temperatures above zero Centigrade, and he made, in addition, the important discovery that for certain metals such as iron and nickel the thermo-electric lines have sudden changes of direction at high temperatures.

The general result of our investigations at low temperatures is to show that, whilst in some cases the thermo-electric lines, as may be seen from the diagram in Fig. 14, are approximately straight lines for temperatures down to the lowest reached, they are not all by any means straight lines. In some cases, such as iron and bismuth, we find sudden changes of direction of the thermo-electric lines similar to those found by Professor Tait at higher temperatures, and this indicates a change in sign in the Thomson effect at that point. Moreover, in many cases there is a decided tendency of the lines of many metals to bend round in a manner which indicates that their thermo-electric power probably would become zero at the absolute zero of temperature.

The temperature at which the thermo-electric line of any metal crosses the line of lead gives us the neutral temperature of that metal with respect to lead, and at that temperature the metal is thermo-electrically identical with lead. If one junction of a couple is at a temperature as far above the neutral temperature of the metals as the other is below it, the couple will give no electromotive force. This provides us with an experimental method of determining the position of certain neutral points. Thus, for instance, if one junction of a platinum-zinc couple is placed in liquid air and the other is raised to above  $30^{\circ}$  we get no electromotive force from that couple. This indicates that the neutral temperature of platinum and zinc is about  $-85^{\circ}$ , and this is shown to be the case from the chart. Two general conclusions are arrived at from a study of the thermo-electric lines as laid down in our chart. The first of these is that the thermo-electric lines of many metals are by no means straight lines over extreme ranges of temperature. Hence the thermo-electric power is not simply a linear function of the absolute temperature. The second important fact is, that in the thermo-electric lines of

certain metals at low temperatures there are sudden changes of direction which indicate a change in the sign of the Thomson effect in that metal at that temperature, and probably, therefore, some important molecular change at the corresponding temperature.

In the case of the 19 and 29 per cent. nickel-steel alloys there is an interesting thermo-electric phenomenon. If a loop of wire of this material is partly dipped in liquid air, the portion cooled becomes thermo-electrically different from the remainder, and gives a strong thermo current if connected to a galvanometer and warmed at one point, where the changed and unchanged portions meet.

Leaving the further elaboration of these points, we must next notice some of the facts with respect to the magnetisation of iron at low temperatures. Professor Dewar mentioned, in a discourse on the scientific uses of liquid air, some results obtained on cooling small steel magnets. These effects we have since again explored at greater length.

Let me show you, in the first place, the effect of cooling a small steel permanent magnet to the temperature of liquid air. We will first take a magnet made of a fragment of knitting needle or ordinary carbon steel and examine the effect of low temperature upon it. Placing the magnet behind the small suspended magnetic needle of a magnetometer we obtain a deflection of the magnetometer needle, which is a measure of the magnetisation of the magnet causing the deflection. On bringing up a small vessel of liquid air and immersing in it the magnet under test we notice at once a sudden decrease in the deflection of the magnetometer needle. This indicates that a notable percentage of the magnetisation of the magnet has been removed. On taking away the liquid air bath and allowing the magnet to heat up again we find that there is a still further decrease in magnetisation. On cooling it again with liquid air the magnetisation then increases, and from and after that time the effect of the cooling is always to increase the moment of the magnet, and the effect of heating it up again always to decrease the moment of the magnet. Hence we see that the effect of the first immersion in liquid air is to give a shock to the magnet which deprives it permanently of a considerable percentage of its magnetism; but when once it has survived this treatment, then cooling it strengthens the magnet, and warming it weakens it.

This is not by any means always the case. If we take a magnet made of the 19 per cent. nickel-steel, the peculiar characters of which were explained a few moments ago, we shall find a very different state of affairs. Here we see the first effect is, as before, to remove a very considerable percentage of the initial magnetisation; but after that stage is passed, then cooling this nickel-steel magnet always weakens it still more, and warming it up again strengthens it. The subsequent effect of cooling is therefore in the opposite direction in the carbon-steel and in this nickel-steel. These changes of moment can best be represented by a diagram of lines as in Fig. 15.

We have in this way examined the behaviour of magnets made of a very large number of steels—chromium-steels, aluminium-steels, tungsten-steels, silicon-steels and nickel-steels, in various states of temper, hard and soft. We find that in some cases there is no initial decrease of magnetism at all, and that the steady state begins at once. Broadly, however, the results amount to this:—A steel magnet when plunged into liquid air generally loses some fraction of its magnetisation, but that after a few such immersions it arrives at a fixed condition in which the effect of cooling it is in most cases to produce an increase of magnetic moment, but in a few exceptional cases to produce a decrease of magnetic moment. In the case of the nickel-steels we have found very curious changes of magnetic

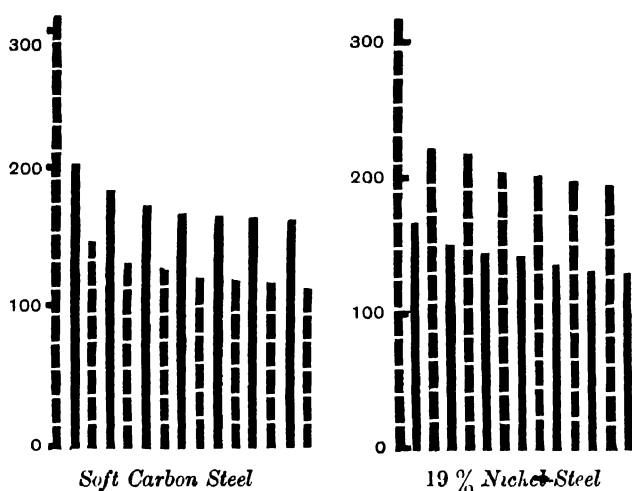


FIG 15

Diagram showing changes of magnetic moment of a magnet when alternately cooled in liquid air and warmed up again to  $+5^{\circ}\text{C}$ . The length of the firm lines represents the value of the magnetic moment when cooled, and that of the dotted when warm.

moment as the magnet is heated up from  $-186^{\circ}\text{C}$ . to  $+300^{\circ}$ . There is a maximum magnetic moment at about  $40^{\circ}\text{C}$ . (see Fig. 16) in the case of the 19 per cent. nickel-steel.

In the technical use of magnets for instrumental purposes they have to go through a process called *ageing* to get rid of the sub-permanent magnetism. One of the best ways of ageing a magnet is to plunge it several times into liquid air.

We have given a large amount of attention to a study of the changes taking place in the magnetic qualities of soft or annealed, and also in hard iron when cooled to very low temperatures.

In the first place, we have examined the change in the permeability of iron at the temperature of liquid air. If a ring of iron is wound over with a coil of wire and subjected to gradually in-

creasing magnetising forces, this force produces magnetisation in the iron, but the magnetisation does not increase proportionally with the force. It tends to a limit, and the curve which shows this variation is called a magnetisation curve. The number which expresses the ratio of the magnetisation to the magnetising force is called the susceptibility of the iron. Instead of considering the magnetisation of the iron as one of the variables, it is often convenient to consider the induction in the iron, and the induction is defined as a quantity, the rate of change of which with time measures the electromotive force set up in a secondary circuit wound round the iron ring.

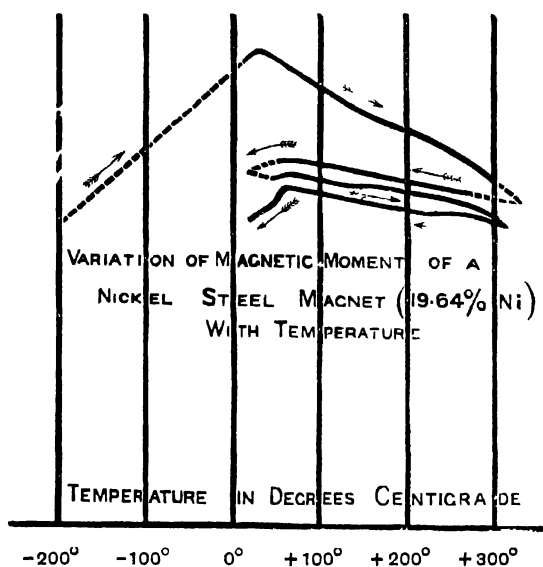


FIG. 16.

The ratio between the induction and the magnetising force at any instant is called the permeability of the iron. By tedious experiments with the ballistic galvanometer, it is possible to draw out a complete magnetisation curve of the iron, starting from the lowest induction up to the point at which the iron becomes practically saturated. Assisted by Mr. J. E. Petavel, who has given us most valuable help in these very tedious magnetic observations, as well as in the subsequent reductions of them, a large number of observations have been made on the permeability of a carefully annealed iron ring made of very fine Swedish iron of the highest quality.\* The result is to show—

\* It is only right to add that in other portions of this work, especially in the resistance and thermo-electric work, we have been much indebted for careful and persevering assistance to Messrs. J. and D. Morris and, in lesser degree, to Messrs. Jakeman and Tilney for help in other observations requiring several simultaneous observers.

as seen from the curve (see Fig. 17)—that cooling the iron to  $-186^{\circ}\text{C}$ . slightly diminishes the permeability. In other words, it requires a greater magnetic force to produce a given amount of magnetisation when the iron is at  $-186^{\circ}\text{C}$ . than when it is at the ordinary temperature.

When, however, we began to study the behaviour of hardened iron in this respect, we found ourselves in the presence of very

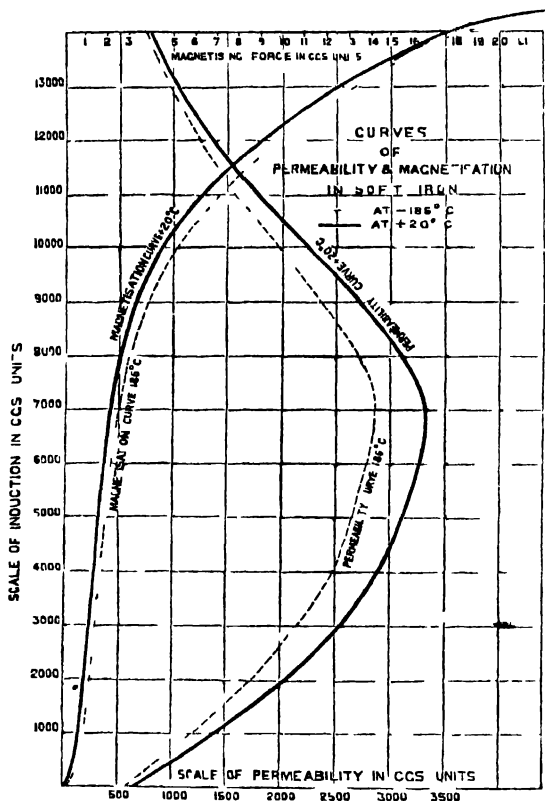


FIG. 17

Magnetisation and permeability curves of soft iron at  $20^{\circ}\text{C}$  and  $-186^{\circ}\text{C}$

curious effects. If pure iron, which has been carefully annealed, is twisted, knocked, bent, stretched, or compressed, it passes into a state known as hard iron, and hard iron has very different magnetic qualities from soft iron. A very extended series of experiments with rings of hard iron have shown that hard iron, at least in certain cases, has its permeability greatly increased by cooling, and this change takes place with great suddenness. We can show you by a simple experiment that this is the case. If we take this hard iron



ring, which has two coils of wire wound round it, and connect one of these circuits to a battery, we shall send a current through this primary coil and magnetise the iron ring. If the other, or secondary circuit is connected to a galvanometer, then at the instant of starting the primary current there is a transitory induced current produced in the secondary circuit. As long as the induction in the iron remains constant no electric change will take place in this secondary circuit. If, however, we plunge the iron ring into liquid air, whilst still keeping the primary current constant, we find again a secondary current produced at the moment of cooling the iron. This indicates a sudden increase of permeability at the instant of cooling. If we bring the ring out of the liquid air we find it retains some of the increased permeability acquired on cooling, but loses a portion of it more slowly if it is heated up again to ordinary temperatures by plunging it into a bath of alcohol. Owing to these changes we found it impossible to repeat again exactly any required magnetisation curve in the case of the hard iron. The sudden cooling alters the magnetic qualities of the unannealed iron to such an extent that it is not possible to get it twice in exactly the same state.

By subjecting a hard iron ring to frequent reversals of the same magnetising force, whilst it is warmed up slowly from the temperature of liquid air up to ordinary temperatures, we have been able to trace the gradual decrease of the permeability at any constant force throughout the results are embodied in the series

We have found, on the other hand, that unhardened steel pianoforte wire behaves like soft annealed iron.

We have then examined the hysteresis of iron at low temperatures. As the meaning of that term was very fully explained by the inventor of it in a discourse given quite recently, no time need be spent in an

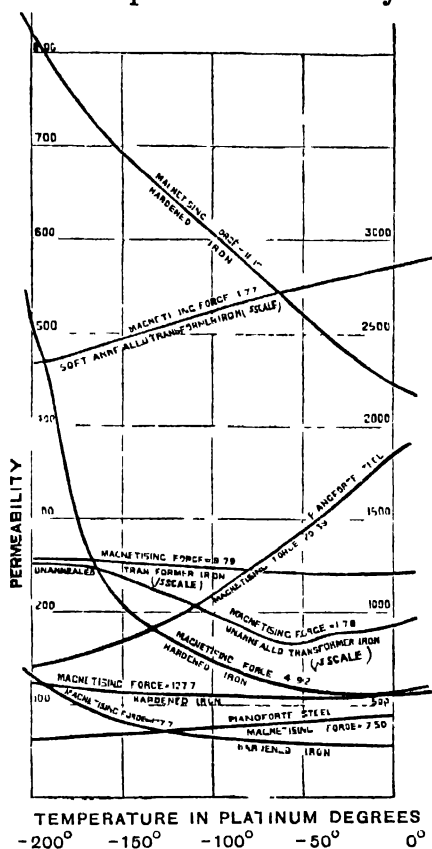


FIG. 18.

Curves showing the variation of permeability of iron with temperature between  $0^{\circ}\text{C.}$  and  $-200^{\circ}\text{C.}$

elaborate explanation of it. It is sufficient to say that when iron is magnetised and demagnetised, or carried round a cycle of magnetisation in which its direction of magnetisation is first in one direction and then in the other, this process involves the expenditure of energy, and such dissipation of energy is spoken of as the hysteresis loss in iron. It would occupy too much time to attempt to explain in full detail the manner in which this dissipated energy can be measured. As a matter of fact, the method we adopted was the laborious but exact one of delineating a complete magnetisation curve of the iron, by means of observations taken with the ballistic galvanometer for various maximum values of the magnetising force. In this way we were able finally to arrive at a curve which represented by its ordinates the value of the hysteresis loss in the iron in ergs per cubic

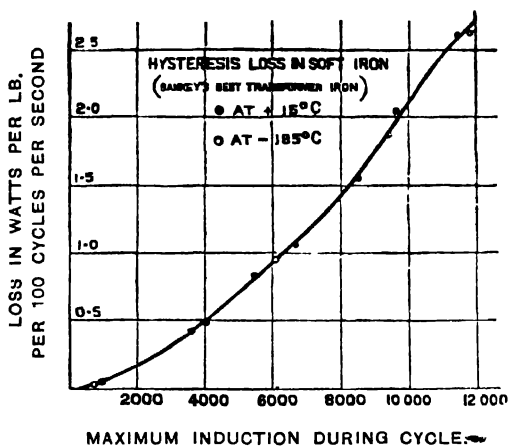


FIG. 19.

Variation of hysteresis loss in soft iron with temperature.

centimetre per cycle, and the abscissæ the maximum value of the corresponding magnetic induction. When curves had been drawn out (see Fig. 19) from all the many hundreds of observations for the case of the same soft iron ring at ordinary temperatures and at the temperature of liquid air, we found little or no sensible difference between them. The result is, then, that there is no appreciable change in the magnetic hysteresis loss of very carefully annealed soft Swedish iron when cooled to these low temperatures.\* With regard to the hard iron, although the permeability is increased, it is most difficult to say yet whether the hysteresis is increased or not, as every fresh reduction in temperature of the iron alters its physical

\* The iron used in all these experiments was a sample of Sankey's transformer iron, kindly sent to us by Mr. R. Jenkins.

state, and makes it almost impossible to obtain similar repeated measurements.

It is natural to inquire how far accepted theories of magnetic action are able to reconcile the above-mentioned results. Some of them undoubtedly are in accord with deductions from received hypotheses. It is generally considered that the facts connected with the magnetisation of iron indicate that each molecule, or perhaps small groups of molecules, of the iron are complete micro-magnets, and that in the unmagnetised condition of the iron these molecular magnets arrange themselves in groups or in closed circuits so that for each little group the external magnetic action or magnetic moment is approximately zero. Magnetisation consists in arranging the members of some or all of these groups so as to co-lineate the direction of more or less of the molecular magnets and produce an external resultant magnetic moment.

Let us then consider one such little group by the aid of a model made of small magnets, such as Ewing has suggested and used.

Suppose the members of this group to be at a certain distance from each other, and we apply a given magnetising force which is just sufficient to open out the group and co-lineate the magnetic axes of the several members of it.

Next, suppose we cool this iron, this would result in bringing the members of the group into closer contiguity. The result of this will be an increase of the interpolar magnetic forces of the different members of the group; and as we can see from the behaviour of the model, it would require a greater magnetic force to effect the same amount of co-lineation of the molecular magnets. This, therefore, corresponds with what we find to be the case on cooling soft iron to very low temperatures. Professor Dewar's experiments have shown that the tensile strength of iron and steel is increased to about double on cooling to  $-182^{\circ}\text{C.}$ , and it is quite reasonable to suppose that this is the result, in part at least, due to an approximation of the molecules.

As regards the behaviour of magnetised steel and iron when cooled, it is highly likely, when the groups of molecular magnets have been opened out more or less, that some of these are in a condition of instability, in which bringing the members of the group nearer together will have the effect of making them close up again into magnetic circuits of no external action. Hence, if this is the case, the first effect of the sudden cooling will be to effect the observed change. These half-hearted groups of molecular magnets constitute the subpermanent magnetism which it is our desire to get rid of in ageing a magnet. Then, as regards the effect of temperature changes on the magnet when the stable condition of affairs is reached. In order to explain this, I think we must consider the action of the molecular groups upon each other. The approximation of molecular groups will in general, after the magnet is aged, have the effect of co-lineating more completely the different members of the groups, and hence increase

the magnetic moment of the magnet, whilst the separation of the molecular groups and the reverse effects ensue on heating. The action of the low temperature upon soft iron and upon magnetised steel would be explicable then if we may legitimately make the assumption that lowering the temperature approximates the molecular groups and also the members of each group.

The result of this, in the case of existing permanent magnetisation, is to close up more or less those groups which are in an unstable condition, but to increase the co-lineation in those groups in which the magnetic moment exceeds a certain value. Hence, in the case of the permanent magnet, the first effect of sudden cooling is a compound effect; it consists in a great reduction of the magnetic moment of certain unstable groups, but in an increase of moment of others. After this initial stage is past, the normal effect is an increase of magnetic moment of the groups by bringing the members of them closer together, and a diminution by increase of distance. There remains then to be explained the anomalous behaviour of the nickel-steel and hardened iron, but an attempt to throw an inner light upon the results obtained with these substances cannot possibly be successful until we have explored far more thoroughly, at low temperatures, the changes in mechanical as well as magnetic qualities.

Much as we may be tempted to speculate upon the causes of these various changes in the properties of matter at very low temperatures, a more important duty at the present time is the collection of facts and the completion of accurate quantitative measurements. The experimental difficulties of this low temperature research are very great, but both Professor Dewar and I have been chiefly anxious in this particular work to prosecute preliminary explorations in as many regions of it as possible, these pioneering experiments enabling us to ascertain in what direction further inquiry will be profitable. Every step forward opens up fresh suggestions for investigation, and, I may add, fresh difficulties. In the light of the results, however, thus ascertained, we shall have additional means of testing and judging existing electrical theories, and the facts themselves, when built into the fabric of scientific knowledge, will serve to broaden those foundations on which we may profitably erect new hypotheses of electric and magnetic phenomena, which, even if they can do but little to dissipate that mystery which enshrouds the most familiar facts, will serve as a continual stimulus to thought and work in days and years that are yet to come.

[J. A. F.]



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## APPLIED SCIENCE